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R. T. EDDISON

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ULTIMATE CAPACITY OF A SLABBING MILL

by

H. G. JONES,* D. T. STEER,* and P. D. DICKERSON*

The policy of integrating iron and steel plants has led to improved functional efficiency and has also led to the concentration of the industry into large units. The question arises whether there are economic or practical upper limits to the size of the integrated plant. Without giving a complete answer, the paper outlines some of the problems encountered in estimating the ceiling capacity of the unit which controls the total output in a particular steelworks.

In practice, in a steelworks the outputs from several individual items of plant converge into a single stream to pass through the slabbing mill. Thereafter the material may either pass to a single line of finishing mills or alternatively to several finishing processes. In either case it is highly desirable to have an accurately established figure for the ultimate capacity of the single unit which determines the total throughput of the integrated plant. The logical procedure, particularly when the single unit is of heavy capital value, is to saturate this unit to its working capacity by multiplying the streams feeding it.

The fluctuations to be expected from hour to hour are also of importance both when considering the equipment to be associated with the mill and also if productivity indices are to be established.

* Steel Company of Wales Ltd.

For the non-steelmakers, it should be explained that the primary or slabbing mill is a pair of horizontal rolls which will reduce red hot ingots of steel weighing up to 18½ tons from a cross section 76 in. × 29 in. to a slab say five in. thick by 70 in. wide with a considerable increase in length. The process takes place in a number of passes, the mill being reversed for each pass; during the process the ingot is also tilted on edge or on flat, to ensure the mechanical working of the steel into an approximately homogeneous material of satisfactory shape.

Although there is a single determining channel, namely the slabbing mill, the possible output is not a continuously variable function, but a function which increases in discrete steps. Thus when this investigation started the output of the Steel Company of Wales Limited stemmed from three blast furnaces. At the expense of over simplification, and omitting the considerable multi-channel intermediate processing, one may say that the problem was first to ascertain the equipment and organization required for the rolling of the additional output from a fourth blast furnace—now erected. It later became evident that there might be a case for a fifth blast furnace; some of the figures quoted refer to this possibility.

Rolling

The existing rolling conditions were examined by a time study of the mill (for the latest of these the teleprinter timing equipment developed by B.I.S.R.A. was employed). Since an essential feature was the need to predict or synthesize the performance when rolling widely different ingots and slabs, the study divided the rolling activities either into constant component times which were substantially independent of the size being rolled (e.g. time to tilt an ingot on its side), or into components which were a function of the geometry at any given stage of the process (e.g. the time in contact with the rolls as a function of the length of the slab).

That this division is justified is illustrated by Figures 1 and 2 which cover ingots ranging in weight from seven to 16 tons at various stages in their reduction to different slabs by various rollermen. No systematic differences were found between these factors.

These results are of some interest, firstly because although clear differences could be detected in the rhythm of the rollermen, no significant differences could be detected in their times to roll ingots. Secondly, a linear relationship is obtained in Figure 2 because the ingot is fed into the mill with the work rolls running at full speed; in other mills it is known that the work rolls are slowed down to receive the ingot. This latter practice gives a non-linear relationship

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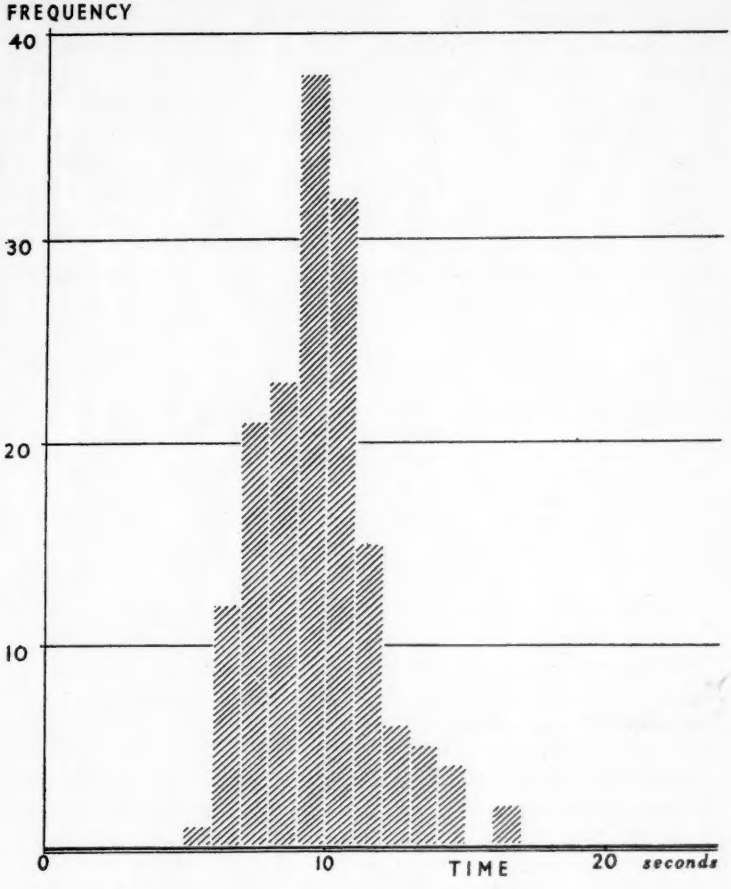

FIGURE 1.—*Time out of rolls with downtilt.*

on Figure 2 and moreover every time is longer than the corresponding time on Figure 2.

At an early stage, it was necessary to decide whether to generalize the results by spreading the observations over a long time or whether to concentrate on an intensely observed period of 48 hours when the steel was going forward at a reasonably steady rate covering the sizes normally rolled, and when one could be certain that the rollerman and observers had settled down to their normal pattern

of work (including fluctuations due to night work). The latter policy was chosen, partly for convenience, but mostly because it was believed that the psychological and administrative advantages outweighed the theoretical disadvantages.

Having obtained component times, the synthesis of the time to roll any ingot to the appropriate range of slabs followed. It was first necessary to decide on the rolling pattern, i.e. the number of passes between tilts and hence the number of tilts which would be required. Figure 3 shows the changes in length, in this particular case, estimated 31 times from the cross section during rolling and the density. A total of 63 elements then enters into the rolling of this particular slab as summarized in Table I.

TABLE I

COMPONENTS INVOLVED IN THE ROLLING OF A 15·4 TON INGOT TO A 31 × $\frac{1}{2}$ IN. SLAB

Total time in contact with rolls	=	65·4	seconds
25 intervals with no tilting	=	55·4	,,
3 down tilts	=	27·6	,,
2 up tilts	=	20·8	,,
Nip on last pass	=	2·0	,,
Time to adjust mill for next ingot	=	13·0	,,
		184·2	,,

Had the mean length been used instead of the length at each stage, the total time in contact with the rolls would have been over-estimated by as much as 35 per cent.

Incidentally, a vital six (of the 13) seconds to adjust the mill between ingots was disregarded at first during the intensive analytical observations; its significance was only appreciated and its characteristics studied during the checking of the synthesized times against several hours working of the mill, when it was realized that the six seconds could not be eliminated. The importance of checking back was thus dramatically demonstrated.

It is of some interest to note that useful work on the ingot (i.e. time in contact with the rolls) only occurs for 35 per cent of the process. The single most time-consuming operation is tilting and its associated operations. A so-called Universal Mill—with a set of vertical rolls just behind the horizontal rolls—could be installed to reduce the number of tilts, and also, by stopping the spread on the vertical sides of the ingot, would save about two passes per ingot. With this slab it is estimated that a Universal Mill would reduce the rolling time by at least 11 per cent.

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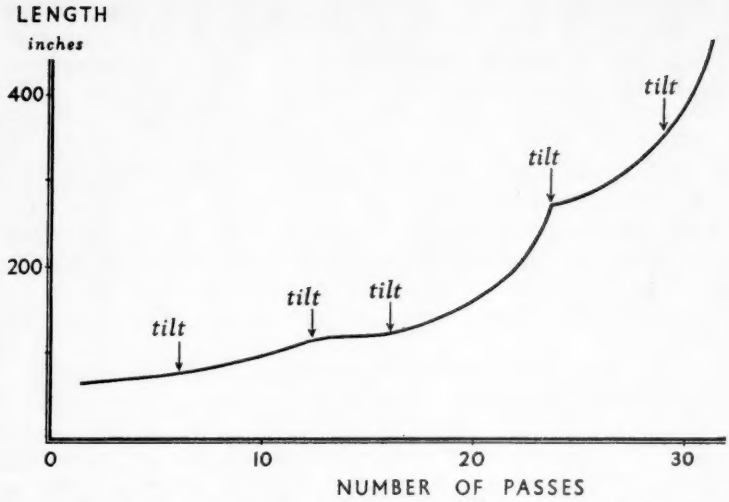

FIGURE 2.—*Time in contact with rolls against length of ingot or slab for single ingots.*

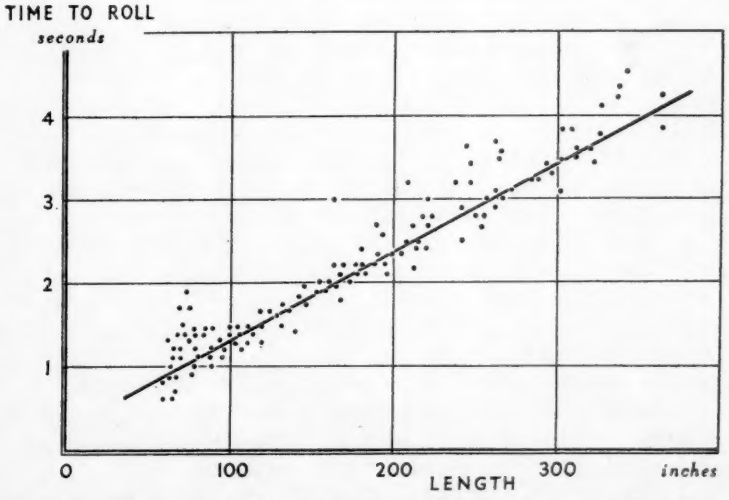

FIGURE 3.—*Calculated lengths of* 15*-ton ingot rolling to* 31 *in.* × 8·5 *in. slab.*

The rolling rate for the slab of Table I in the existing mill works out at 300 tons per hour, provided there are no stoppages in the process. The most obvious necessity in utilizing the rolling capacity to the full is that the ancillary equipment should be capable of supplying the steel at at least the rolling rate.

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Serving the Mill

The steel is prepared for rolling by soaking at the desired temperature, batchwise, in a line of furnaces close to the mill. Each ingot is lifted from the soaking furnace by a crane, fitted with dogs or tongs, and carried to the mill on a four-wheeled carriage known as a "buggy" which incorporates a device for tilting the ingot from a vertical to a horizontal position ready for rolling.

It is proposed to trace in some detail the steps taken in examining the buggy as an illustration of the need to preserve a flexible outlook.

Time studies of the operations enabled the delivery rate of the 15-ton ingot from the different soaking furnaces to be estimated as shown in Figure 4. (The stepped nature of the curve is due to furnaces being in pairs.) There will be no delay on single rolling with steel from any pit but with tandem rolling (i.e. two ingots taken through the mill pass by pass one behind the other), the mill will be waiting for steel when pits 21 to 30 are in use.

If a lighter ingot, such as an 8-ton ingot being rolled to a 31 × 6 in. slab, is examined the position is not so happy. A fairly high rolling rate of 235 tons per hour can be maintained with single ingot rolling, but the supply of steel by the buggy is considerably slower (in the ratio of 8 : 15; curve (a) on Figure 5) and delays will occur with steel from pits 13 to 30. Moreover, this ingot is generally rolled in tandem at a rate of 335 tons per hour when the present buggy is incapable of serving the mill fast enough from any pit.

Since component times are available, the effect of various modifications such as speeding the travelling time or the tilting time, or reducing the length of travel by extending an existing roller table towards the furnaces, can be estimated with some accuracy. Further possibilities which have been evaluated are the provision of a trailer so that two ingots could be carried, and also the breaking down of the duties so that the present buggy (located opposite the furnace being drawn) acts merely as a tilting device and transfers the ingot to a simple shuttle carriage for transfer to the mill.

The delivery rates obtained with these alternatives are also shown on Figure 5.

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It is of some interest that the "flat steps" occur when the buggy determines the supply rate—the "saw teeth" occur when the crane is an additional limiting factor.

The cost of installing these alternatives by chance increases with their effectiveness and becomes very considerable with the roller table, for instance, because of either the downtime or the reduced rolling rate which would be forced on the mill during the modifications. There is therefore a considerable urge to employ the cheapest system which is adequate.

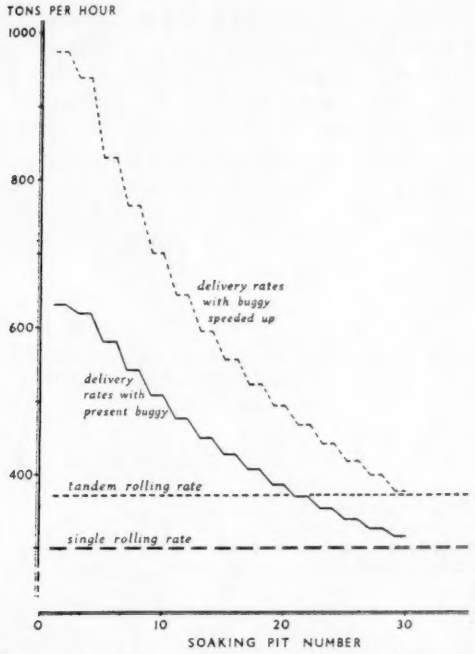

FIGURE 4.—*Delivery rates from different soaking pits.*

The superficial conclusion from Figure 5 is that alternatives (a) to (d) are hopelessly inadequate for "light" ingots and should not be employed. Further examination, however, indicates that this is a superficial conclusion requiring some modification.

Market research undertaken by the Company suggests that the weekly tonnage of the 8-ton ingot would be such that it could be rolled in 8·6 hours. Similarly the total rolling time for all "light" ingots, difficult to supply to the mill fast enough, is estimated to be

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only 23 hours per week. Is it then worthwhile embarking on extensive capital expenditure to keep the mill working at its maximum capacity throughout the week? Or is there a satisfactory compromise? Some of the Americans describe the process of evaluating the compromise as "overall optimization".

Table II shows the total weekly times required to roll the predicted tonnage when the mill is served by alternative delivery methods. The buggy-speeded-up then looks at first sight to be promising, but further examination again causes one's opinion to be revised.

In passing, it might be mentioned that these times were estimated after ensuring that the optimum size of ingot was being used for the production of a given slab.

It has also been estimated that a 20 per cent increase in speed of the mill would reduce the rolling time by about 11 hours but the extra wear and tear and consequent increased maintenance might easily lose more than the rolling time saved. A further 20 per cent increase in the speed of the mill would only save a further one hour in rolling time, because of the greater difficulty in controlling the ingot.

TABLE II

TIME TAKEN TO ROLL THE EXPECTED WEEKLY TONNAGE UNDER VARIOUS CONDITIONS

System	Time to roll (hours)
Perfect delivery	132
Present buggy	166
Present buggy (speed increased)	146½
Buggy and trailer with 1 charger crane	139
Buggy and trailer with 2 charger cranes	136½
Buggy (tilting device only) and shuttle with 1 charger crane	134½
Roller tables extended to between pits with faster buggy:—	
1 and 3	139
3 and 5	137
5 and 7	135½

Downtime

Steelworks normally work 168 hours a week, and so the further question may be asked of whether it is justifiable to embark on capital expenditure to reduce the operating time from say 146 hours per week to 134 hours.

At present, a complete shift of eight hours a week appears quite essential for roll changes (even after some redesign) and for the routine mechanical and electrical maintenance which is essential in a machine doing heavy work in hot and abrasive conditions.

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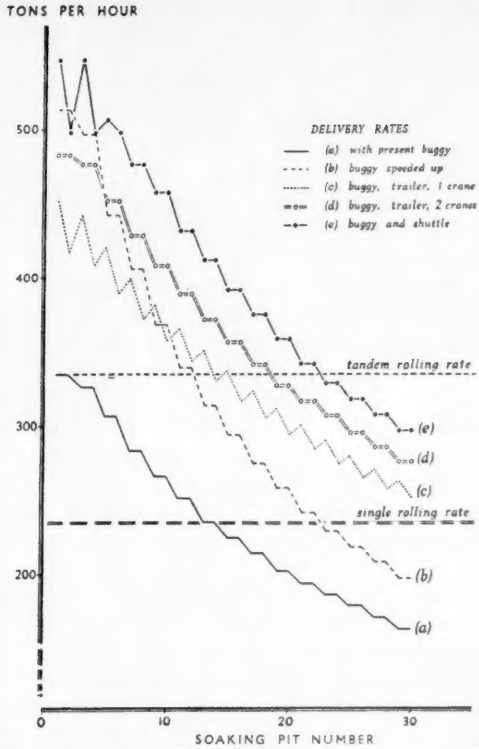

FIGURE 5.—*Delivery rates from different soaking pits under modified conditions.*

Moreover, the same conditions superpose further inevitable delays due to minor breakdowns; also, departures from the idealized process are necessary from time to time in what is essentially a batch process, in order to maintain a consistently high standard in the product. Can a firm figure be evaluated for these delays?

Four distinct approaches have been made and all have failed.

The routine records of major breakdowns over 12 months have been analysed by the Engineering Department; the immediate outcome has been the revision of maintenance standards and spares. Important as the revision is, it makes past records of doubtful value for predicting the future.

The ratio delay technique fails to give a clear picture because the mill is at present, even on the very high tonnages compared with other mills throughout the world, working so far below its ultimate

capacity that stoppages for steel are utilized for maintenance or production tasks (such as removal of pieces falling off the ingot). On other rare occasions, steel might be delayed for minor maintenance either on the mill or further down the production line. It is difficult, under these conditions, to be really sure of the facts with the spot ratio delay approach.

Five complete shifts have been studied, using stopwatches to time delays. But for one reason or another, in practice the information was never complete in respect of minor delays.

The best approach so far as been intensive observations with the teleprinter timing equipment on peak shifts. In the three shifts studied in this way, all the relevant factors were recorded in detail and a comparison then made between the theoretical estimates and the actual observations. Deviations were allocated to production delays, or to modifications in the ideal rolling policy, or to breakdowns. In this way, the 480 minutes in a shift could be accounted for to within eight minutes. It was found that minor delays of an engineering nature totalled between five and 10 per cent of the time, but what value have three shifts, even peak shifts? (The same order of delays was suggested by the stopwatch and ratio delay observations.)

The standard error in the estimate of 132 hours to roll is small (one hour), but the importance of considering the standard error diminishes when delay factors of nine to 10 per cent are arbitrarily introduced on scanty evidence. But under the particular circumstances what alternative approaches are there for better estimates?

Conclusions

It is now appropriate to assess the position. Studies of the rolling process suggest that the output from four blast furnaces could certainly be handled in the existing slabbing mill provided the serving devices underwent some modification to permit a working margin of safety.

For five blast furnaces, the idealized process of rolling requires 132 hours of the week in the existing slabbing mill; it is possible to supply the steel at approximately this rate by comprehensive review of the serving arrangements and provided no link in the supply chain breaks. Had the steel been available in adequate quantities and had the present serving devices been adequate to yield representative conditions of future performance, then determined efforts would have been made to estimate the probable delay to each factor entering into the week's output. As it is, one can

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only say that, of the remaining 36 hours in the week, at least eight hours will be required for routine maintenance and at least a further 13 hours will be lost due to minor breakdowns. Fifteen hours' margin a week are left for untoward incidents, minor (but cumulative) delays at shift changes, reliefs for meal breaks, queries to the rollermen, changes in the rolling programme or indeed changes in the customers' requirements. With this narrow margin, a six-hour delay superposed by the buggy-speeded-up could not be tolerated. Even with no delay at the mill waiting for steel, the position seems hazardous.

A solution is to modify the present mill by conversion to a Universal Mill when the output corresponding to five blast furnaces could certainly be rolled (theoretically in about 110 hours) and possibly that from even a sixth blast furnace.

After that limit, with present practices, a duplication of the works appears necessary on either the same or a new site. It is suggested that the availability and handling of the raw materials (as well as the future markets) then become the dominating factors and, interesting as these have been to examine, they are outside the scope of the present paper.

To sum up, it appears that the time study of the rolling process and the combined time and motion studies of the various serving devices have given an idealized but helpful quantitative estimate of the weekly output. Although the latter stages of the investigation can certainly be criticized by pedantic purists, the empirical attack seemed appropriate under the conditions pertaining at the time.

ACKNOWLEDGMENTS

The Directors of the Steel Company of Wales Limited are thanked for permission to publish this paper.

Although described from the operational research angle, the investigation has obviously been very much a team job and it is a pleasure to acknowledge the co-operation of all levels of the Mill Production and Maintenance staffs.

This paper was presented to the Operational Research Society *on October 13, 1955.*

COMPUTERS AND O.R.

THE Operational Research Society held its annual open meeting in London on 9 March, the subject being *The Influence of Data Processing Machines on Operations*. Sir Charles Goodeve, F.R.S., in the Chair, suggested that the introduction of data processing machines

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marked as significant a mathematical step forward as the introduction of the zero sign a thousand years ago. The influence, however, was on operations in the later case, rather than on calculation. The discussion was opened by Dr. B. V. Bowden (Principal, Manchester College of Technology), Dr. S. H. Hollingdale (R.A.E.), and David Owen (British Iron and Steel Research Association). Subsequent speakers from J. Lyons, I.C.I., the G.P.O., the Ministry of Insurance, the N.P.L., London Transport and other organisations took part. A report will be published in our June issue.

A PROBLEM OF REPLACEMENT

by

K. PENNYCUICK*

THIS PAPER is concerned with the signal equipment of an army division. The original data is reproduced at Appendix A. The data suggests that the equipment was wearing out. Some of it was certainly showing decreasing intervals between successive visits to the divisional workshop for repair. The object of the analysis which follows is to estimate the number of new sets to be provided in order to hold the repair rate reasonably constant. The data is anonymous, i.e., there is no reference to the division whose equipment was studied, nor is there any reference to the name of the equipment. The paper is published by the permission of the Scientific Adviser to the Army Council.

Scrutiny of the data suggests that the unserviceability rate is subject to seasonal fluctuations. Accordingly, the parameter investigated is p, the percentage increase in the unserviceability between periods a year apart.

With the above approach it is important that the total amount of available equipment of each type should have been constant over the period under review. This condition is believed to have been effectively true for the 22 four-week periods considered.

Replacement Theory

Action must be taken to compensate for the increase in the unserviceability rate and the method adopted is to replace some sets by new ones. The question next to be posed is how many new sets and what effect will the provision of new sets have.

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* The author is greatly indebted to Mr. R. J. Taylor for preparing for publication this shortened account of the original work.

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In order to answer these questions satisfactorily, some knowledge of the distribution of the period between repairs, both for existing equipment and new equipment, is required. "Existing" in this connexion implies sets in use and "New" implies sets not yet taken into use. The knowledge required is not available but it is possible to get some idea of the effect produced by an injection of $n\%$ new sets, by taking a hypothesis at the upper and lower limits of what seems reasonable.

Consider the ratio of the mean life (m_1) of the worst $n\%$ ($n = 100w$) of sets to the mean life (m) of the existing sets. Let this ratio be r_n. Then the mean life of the best $100(1-w)\%$ sets under prevailing conditions is

$$\frac{(1-wr_n)}{1-w}.m$$

and the injection of $100w\%$ of new sets decreases the number of sets coming in for repair each period by the factor $(1-w)$

$$\therefore \quad m_2/m = \frac{(1-wr_n)}{(1-w)^2}$$

where m_2 = mean life of the stock after replacing the worst $100w\%$ of sets by new sets.

Some idea of the relation between w and r is now required. Clearly the worst case is when all sets are equally bad, i.e., $r_n = 1$ (for all n). This is termed the worst case because injection of new sets has the least effect.

The injection of new sets has most effect in the degenerate case when $r_n = 0$ (for all n), i.e., when all existing sets are useless, but normally r_n decreases as n increases and w never exceeds r_n: hence best effect occurs when $w = r_n$.

In practice something between the two may be expected, say $r_n = (1+w)/2$. Table I shows the ratio in which new life may

TABLE I

INCREASE IN MEAN LIFE

Ratio r_n = Life of Worst $n\%$ of Sets / Life of Existing Sets

$n\%$ of New Sets	0·1	0·2	0·3	0·4	0·5	0·6	0·7	0·8	0·9	1·0
5	1·10	1·10	1·09	1·09	1·08	1·07	1·07	1·06	1·06	1·05
10	1·22	1·21	1·20	1·19	1·17	1·16	1·15	1·14	1·12	1·11
15	—	1·34	1·32	1·30	1·28	1·26	1·24	1·22	1·20	1·18
20	—	1·50	1·47	1·44	1·41	1·38	1·34	1·31	1·28	1·25
25	—	—	1·64	1·60	1·56	1·51	1·47	1·42	1·38	1·33
30	—	—	1·86	1·80	1·73	1·67	1·61	1·55	1·49	1·43
40	—	—	—	2·33	2·22	2·11	2·00	1·89	1·78	1·67
50	—	—	—	—	3·00	2·80	2·60	2·40	2·20	2·00

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be expected to increase above the current level on replacement of $n\%$ of present sets.

The case $r_n = 1$ implies removing $n\%$ of existing stock by random choice. It is assumed that

(*a*) Life of new sets is large compared with old sets. This assumption becomes unreal for large values of n.

(*b*) That the mean life of the $n\%$ of sets removed is not less than $n\%$ of the mean life of the existing stock.

Application of Theory

Set A. Set A requires prompt attention. The deterioration equation (see Appendix B)

$$p = 32 + 8{\cdot}75t \qquad (1)$$

where t is the time parameter, a unit representing a four-week period, shows not only that the equipment is deteriorating rapidly but that the deterioration rate is increasing rapidly.

Equation 1 shows that the mean life of Set A has decreased by 32% over the past year and appears to be decreasing rapidly. If it is required to restore the repair position to where it was a year ago the existing mean needs to be increased by a factor of $\frac{100}{100-32} = 1{\cdot}47$. Reference to Table I shows that this increase is ensured by replacement of any 30% (say) of the sets. Careful selection of the sets removed might produce the same effect for injection of 25% or even 20% new sets. Allowing however that it will be some time before new sets arrive the figure of 25% appears sound. ($v_n = 0{\cdot}7$.)

Since there is a deterioration of 32% in life over a year it would appear that a continuous injection rate of 25% new sets a year is also required. In terms of present stock this implies (say) 120 new sets for soonest delivery and a continuous injection of 10 new sets per month. These are minima. It is desirable (allowing for lag before deliveries commence) to order 150 new sets now and add fresh stock at the rate of 12 sets a month. (See below—discussion.)

Set B. The state of Set B is relatively worse than that of Set A, for the deterioration equation (see Appendix B) is:

$$p = 14 + 8{\cdot}95t \qquad (2)$$

and this is unlikely to occur by chance. This equation implies that the deterioration rate of these sets (2) has started to increase rapidly and that the mean deterioration rate over the current period is almost 100% (i.e., set life has halved). This implies an urgent need for 50% new sets on a basis of those in use with units to restore the

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state of a year ago and an injection to replace the remaining sets over about two years. The evidence is not good enough to predict what will then happen.

In terms of existing stocks this implies 60 new sets and a delivery rate of 5 sets a month.

Set C. There is no evidence of rapid deterioration in the condition of Set C. There is reason to suspect slight deterioration, but this does not exceed 10% per annum and may well be less (the calculated figure is 6%). In view of the number of these sets issued it appears that 10% new sets per year should provide an adequate replacement rate and this can be regarded as met by a replacement rate of 3 sets per month.

Sets D and E. There is no evidence of increasing deterioration—if anything the evidence suggests that the condition of these sets is now improving no doubt owing to lack of use for other reasons.

Unless these sets are taken into more frequent use there is no necessity to obtain fresh supplies. The evidence however suggests that if these sets are in heavy use, the replacement rate should be based on a five year turnover, i.e., five per month. This is based on a mean annual deterioration rate of 25% and a replacement rate of 20%.

Set F. The number of these sets coming to divisional signals for repair is not in itself sufficient to cause concern. There is, however, reason to believe that the mean life has decreased somewhat in the period under review, but this may well be a random fluctuation. The mean life of the present stock between repairs at divisional level is high and it is not thought necessary to advocate any firm replacement policy. It should be sufficient merely to replace sets destroyed.

Discussion

The data suggest that the conditions of these sets is deteriorating. Let us consider the manner in which this may happen.

A set comprises of frame, various electrical components such as valves, condensers, resistances, switches and holders and wiring. This wiring is insulated and generally joined to components by solder.

If a set breaks down it will usually be due to either: (*a*) faulty component, (*b*) broken joint, (*c*) insulation failure. It is realised that mechanical destruction can occur, but this should be rare.

Now the first two faults are easy to rectify once detected. The third is a longer but rarer fault, but again can be rectified without too much trouble. From all accounts the main cause of set failure is failure of a component.

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Presumably once a set is taken into service the individual components will either fail soon (some valves) or go through their useful life. Some overloaded valves will fail frequently. But most components will continue to function for a lengthy period. Sooner or later however they will start to fail and when this happens a set is liable to give continuous trouble until all failing parts have been removed. Then short of joint or insulation trouble it should take on a new lease of life.

One might predict therefore that any batch of sets will tend to deteriorate slowly until components start failing. There will then be deterioration in condition, which deterioration may increase rapidly, stabilise and then improve.

The phenomenon of increasing repair rate may however be self aggravating. A sudden rush of sets for repair may result in hasty or inadequate work which in its turn results in still more frequent visits to workshops for further repairs.

It is suggested that for the majority of sets in this particular division rebuilding would be much more economical than replacing components piecemeal. In the meantime a vigorous policy of return to base as beyond local repair seems to be required. If the hypothesis outlined above as to the cause of deterioration is correct then the rate of injection of new sets will require alteration from time to time.

Summary of Conclusions

Set A. In order to arrest deterioration of these sets a minimum of 120 new sets is required with an injection rate of 10 new sets a month thereafter. In order to allow for delays in delivery it is desirable that 150 sets be ordered with a further delivery of 12 new sets a month additional to any replacements now being delivered.

Set C. Similarly for Set C an urgent delivery of 60 new sets is required, with a further delivery of 5 sets extra a month.

Set B. An injection of 3 new sets per month should be sufficient to avoid rapid deterioration, and eliminate the present slow deterioration.

Sets D and E. If this set is taken into more frequent use (it is understood that the present performance is unsatisfactory and therefore the set is not being used to any great extent) a replacement rate of 5 per month seems adequate. Otherwise the repairs seem to be slowly overtaking breakdowns.

Set F. Provided a reserve stock is maintained and replenished as required owing to physical loss or destruction, no other action is indicated.

APPENDIX A

THE BASIC DATA:—NUMBER OF SETS SENT TO DIVISIONAL WORKSHOPS FOR REPAIR IN SUCCESSIVE FOUR WEEK PERIODS

Period	Set	Repair Rate A	B	C	D	E	F
1		94	41	111	50	43	52
2		68	46	140	32	25	42
3		124	42	126	38	39	41
4		103	42	104	44	19	53
5		56	36	108	26	25	54
6		103	26	96	38	31	41
7		67	38	88	41	50	95
8		82	42	127	49	34	59
9		112	45	160	61	71	35
10		125	43	176	46	61	44
11		140	43	160	70	62	57
12		146	59	145	71	49	82
13		176	77	186	69	36	114
14		125	56	141	61	39	54
15		125	50	154	52	43	71
16		168	56	114	40	20	55
17		161	62	122	42	37	65
18		100	57	50	45	36	72
19		112	58	111	48	36	54
20		159	59	122	75	54	68
21		199	65	141	57	51	83
22		212	93	154	51	48	59

APPENDIX B

	A	B	C	D	E	F
Number in use (less those under repair)	500–600	100–150	400	300	300	2,500
Mean current life	13 wk.	8 wk.	11 wk.	5–6 mth.	7 mth.	3 yr.
t	(% increase "p" for 4-week period "t")					
1	33	37	27	22	−10	4
2	84	9	10	62	72	69
3	52	33	−10	5	−49	34
4	56	48	18	−5	95	23
5	79	58	−54	73	44	33
6	9	123	14	26	16	32
7	137	55	39	83	80	−20
8	143	55	11	16	50	41
9	89	107	−4	−16	−32	73
Regression	p = 32+ 8·75t	p = 14+ 8·95t	p = 8− 0·42t	p = 38 − 1·7t	p = 27 + 0·42t	p = 25 + 1·28t
Correlation coefficient	0·537 non-sig.	0·685 approx. 3% sig.	0·004 non-sig.	0·13 non-sig.	0·02 non-sig.	0·11 non-sig.
Mean number sent in for repair per 4-week period —over whole period	126	52	129	50	40	61
—over last 6 months	157	66	117	53	44	67

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CONFERENCE REPORTS

International Statistical Institute

AT THE 29TH CONFERENCE of the International Statistical Institute held at Rio de Janeiro in June, 1955, there was a session on "The Place of Statistics in Operations Research". In a paper entitled *Quality Control and Operations Research* Paul S. Olmstead described Q.C. and O.R. as procedures of decision and the steps of O.R. as being (1) the postulation of a model of the operation, (2) the determination of the results given by the model when applied to the existing situation, (3) a comparison between these and the corresponding actual results, (4) the formation, from this comparison, of a judgment as to the appropriateness of the model, and (5) the use of the model to predict the "best" arrangement for the operation. There were parallel steps in quality control, and indeed in the processes of specification, of inspection for conformity to a specification, and of scientific investigation in general.

In his paper *Statistics and Operations Research* Professor Philip M. Morse examined the nature of O.R. in some detail, taking as an example a theoretical study of automobile traffic flow and leading on to queueing theory and linear programming. He concluded: "As yet we have not needed to use high-powered statistical methods in analysing our experiments. If we need more data, it is usually better and safer to devise another experiment than to re-analyse past data. Nevertheless, we do need statistical techniques in our work, and it behoves us to keep abreast of the latest developments in this field, as well as in game theory, communications theory, cybernetics and many others."

R. T. Eddison, B. H. P. Rivett and E. C. Williams chose to deal with their subject, *Statistics in Operational Research in the United Kingdom*, largely by describing six particular pieces of work that have been or are to be published in *Operational Research Quarterly* or *Applied Statistics*. They said that the methods of O.R. can be grouped under three main headings: (1) empirical methods, when laws are derived from observed data, (2) the theoretical approach, in which the law describing the operational situation is derived from theoretical reasoning and is confirmed by observation, and (3) the quasi-empirical approach in which the expected law is derived from some model or analogy.

The discussion, like many general discussions on O.R., turned on the nature of the subject. All shades of opinion were expressed from the view that O.R. is an integrated subject standing in its own right and using various techniques at need, to the view that O.R. is

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nothing more than the name given to an unorganized collection of techniques taken from other disciplines. The prevailing view seemed to be that statistics, although very much used in O.R., does not dominate the subject.

As far as the International Statistical Institute is concerned the conclusion was drawn that O.R. is an activity, as are quality control or market research, in which statistics are widely used and that the ISI may consequently consider the discussion of O.R. and its specific aspects as a suitable subject for discussion at its meetings.

L. H. C. T.

Copenhagen Conference

The "First International Congress on the Application of the Theory of Probability in Telephone Engineering and Administration" assembled in Copenhagen from 19 to 23 June, 1955, at the invitation of the Copenhagen Telephone Company (KTAS). Despite the absence of the word "traffic" from the title, most of the papers would have been described by O.R. practitioners as "traffic studies", though they dealt mainly with applications of Erlang's methods to specific problems peculiar to the telephone industry.

Some 25 papers were presented during the Congress, and many dealt with problems in some degree analogous to those found in other industries. Such papers as *Some Delay Problems Relating to a Full-availability Group Loaded by Two Sources of Traffic* by J. W. Cohen (Phillips, Holland) lead one to speculate whether it may be preferable to devote effort to showing such problems in the more general form, that is, with n sources of traffic, thereby opening the way to a wider field of industrial application. Frequently, of course, the general problem is mathematically intractable, and since Erlang's method, assuming statistical equilibrium, requires only relatively simple mathematics for the particular case of random traffic flow, it may be more profitable for the individual to obtain solutions for a few special cases.

Variations in traffic intensity are discussed by L. von Sydon (L. M. Ericsson Co.), and a more general method is used in a paper *Sur le calcul de la probabilité de perte* by Prof. R. Fortet (Institut Henri Posincaré). Another paper which is recommended to mathematicians studying congestion problems in industry is *Analogies Between Congestion and Communication Theories* by R. Syski (Hivac Ltd.). This is an extremely good survey of the whole field of stochastic processes, and contains an excellent bibliography with references to a great variety of applications.

Papers by A. Jensen (KTAS) and G. Lennbach discuss the applicability of decision theory in the planning and operation of telephone plant, and a method of determining the optimum size of the plant is described. This work may well find uses in other industries.

As in other contexts, the prediction of delays (or lost calls) in telephone traffic is being attempted by sampling experiment as well as by analytical methods. A number of special purpose "traffic-analysers" have been built and in Holland an electronic computer is to be built for the main purpose of pursuing the simulation method of attack. A group of papers by G. Neovius (L. M. Ericsson Co.), L. Kosten (The Hague), S. Ekberg (Swedish Board of Telecommunications) and S. A. Karlsson (Helsingfors Telephone Co.) describe work in progress. Perhaps one of the most interesting issues in connexion with congestion problems is whether analytical formulation or simulation holds out more promise in particular cases: either approach has at some point to sacrifice realistic detail for expediency.

Only papers which are thought to be of wide value outside the telephone industry have been singled out in this brief account. The Congress as a whole, however, provided considerable interest from the O.R. point of view, and was a welcome opportunity for contact with some of the people studying telephone traffic problems.

R. R. P. J.
D. G. O.

British Institute of Management

THIS CONFERENCE was held at Harrogate on 2–4 November, 1955, on the theme of the impact of science on management in the future. No attempt was made to limit the conference to particular lines within this very broad theme and the 24 main papers delivered at four main sessions each divided into six concurrent sections tended to present a picture of odd points of impact rather than of a continuous relationship.

Some papers dealt with the effect of science on what has got to be managed (e.g., the development of production tools, the uses of radio-active isotopes); others on the scientific development of the tools of management (e.g., operational research, electronic processing of data, market analysis, accounting procedures); others again on training and recruitment of managers, health, co-operative research and other miscellaneous subjects.

The conference was attended by some 800 representatives from a very wide range of industries and with many different interests

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within those industries. Such a gathering provided a unique opportunity for renewing old friendships and making new ones, and there can be few who left it without having succeeded in their search for new ideas in the particular aspects which interested them. One cannot help wondering, however, whether an even better purpose would have been served by a conference with a more tightly integrated programme, with a little more depth and a little less breadth. R. T. E.

ABSTRACTS AND REVIEWS

Iron Ore Carriers for Britain.
CHARLES GOODEVE and J. S. TERRINGTON (B.I.S.R.A.)
Shipping World, 19 October, 1955; **133** (3251), 371–377.

The authors review the effect on the shipping requirements for iron ore of the planned increased output of the British steel industry. The case for using specialized iron-ore carriers rather than tramp general cargo vessels is examined with particular reference to carrying capacity in relation to draught, operating costs and unloading times. The depth of water available at British ports is examined and the relationship between draught and deadweight for existing carriers and general cargo ships is reviewed.

From this, the optimum sizes and design features of ore carriers for the British trade are determined.

Operational Research.
R. T. EDDISON (B.I.S.R.A.)
J. Inst. Actuaries Students Soc., July, 1955, **13** (3), 174–184.

A brief introductory discussion of the subject considering in general terms what it is, why it is needed, what it tries to do, how it sets about it and how it can be organized.

Optimum Use of Scattered Store Houses.
A. H. SCHAUFSMA.
Tydschrift voor Efficientie en Documentatie, 1955; **25**, 229. (*In Dutch.*)

The author analyses an existing condition of scattered store houses all of varying nature, with different handling facilities and a variety of stored material. The article shows how this type of analysis may be used to reach decisions as to the best use of available store space. The method also allows an analysis of the possible savings for the case that a new store house is contemplated to replace one or several of the existing ones.

Handbook of Industrial Engineering and Management.
W. GRANT IRESON and EUGENE L. GRANT.
Prentice Hall, 1955, 1,203 pp. 128*s*.

This is an expensive book which contains much that is of importance to the management trainee or potential industrial executive. It contains 17 chapters, each covering a different aspect of modern industrial operation. Together, they form a "milestone" volume, summarizing the state of the art to-day, rather than adding up to a practical guide.

Much of the material is covered at a somewhat advanced level. The presentation either presupposes a high standard of knowledge on the part of the reader or assumes that he has already read a previous chapter. The book cannot therefore be considered as a quick reference work for top or medium level management.

Chapter 15, *Industrial Operations Research*, by A. W. Swan (which is the only non-American contribution) is a first-rate review paper on the subject, though it seems somewhat out of place in a volume intended as a permanent reference work. O.R. techniques are not described in detail, the subject being dealt with mainly in the form of case histories. Mr. Swan has written an excellent introduction, embodying the general concepts and principles of O.R., and has wisely selected a group of case histories which are new to the literature. These are interesting in themselves and cover a wide selection of topics from *The Economic Control of a Book-Manufacturing Plant* (Littauer) to *Obtaining Maximum Efficiency from a Barber-Colman Knotter* (Sondhelm). The case histories are all too brief, and it would be interesting to locate further details.

Many of the other chapters are also of interest to the O.R. worker. The chapter on *Industrial Statistics*, for example, is a good elementary reference work on statistical quality control. It is followed by a chapter on *Inspection and Quality Control*, which deals with the operation of quality control in an industrial establishment. The section on *Motion and Time Study* is one of the few "handbook" articles in this volume. Other subjects covered, which are of more than passing interest to the O.R. worker, include: *Industrial Budgeting*, *Engineering Economy*, *Managerial Economics*, *Trade Unions*, and *Industrial Engineering*.

All of these chapters can be regarded as introductions to some of the broader managerial and planning concepts in industry and in organization. For the O.R. worker, however, the volume as a whole has only a limited value as a "handbook". It would indeed be a pity if, owing to its inclusion in this book, the valuable review of operational research were to receive insufficient attention. E. K.

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Management Science.

Institute of Management Science, New York.

Readers of this Quarterly are familiar with the work of the Operations Research Society of America, and in particular with their *Journal*. What may not be as widely known is the existence of another similar Society in the U.S.A. namely, The Institute of Management Sciences. The Institute was formed in 1953 with the objects "to identify, extend and unify scientific knowledge that contributes to the understanding and practice of management". The Institute was set up with only one class of membership, in order that a whole cross section on management and of scientists interested in management problems might equally find a forum for discussion of mutual problems. The Institute has published from October, 1954, a journal *Management Science* and the purpose of this article is to review the four numbers which comprise the first volume.

It is at first sight difficult to see how management science as such differs from operational research. An interesting article in the first number of this volume is devoted to an exposition of an evolution of a science of management in America. Most of the work quoted in this survey as contributing to the evolution of management science, in particular that of Taylor and the Gillbreths, is equally understood as being the precursor of operational research. A similar confusion of a possible difference between management science and operational research is shown in a number of articles in the journal. The view of the writer of this review is that if there is a science of management as such, it will be one which must have as component techniques, work study, job evaluation, time and motion study and, in a rather broader context, operational research. It is against the criteria of whether this distinction between management science and operational research is made sufficiently vivid that the work of the new Society as revealed in its journal must be assessed.

In a policy statement for *Management Science* the managing editor, Dr. C. West Churchman, says that the journal will be publishing the following types of article: (1) Evaluative articles which are designed to portray, explain and assess developments in fields which are relevant to the management sciences. These articles will be expressed in non-technical language so that they will be readily accessible to persons in managerial positions; (2) method articles which are designed to explain the methods that scientific research has made available for various kinds of management problems. These articles will be expressed in a language which is readily accessible to engineers and scientists who are interested in applying

these methods to practical problems; (3) survey articles which are designed to assess selected aspects of various fields which are or can be relevant to the management sciences. These articles are intended to provide guidance for research workers in the management sciences. As such they may be expressed in technical scientific language modified in a manner which will make them suitable for persons who do not possess backgrounds of specialization in the fields being reviewed; (4) technical articles which are designed to present the results of original research in theory or application. These articles will be directed toward persons who are intimately concerned with such research. Technical terminology may be utilized in these articles.

Dr. Churchman goes on to say that articles from any field will be welcomed as long as the article furthers the aim of developing a unified science of management.

It must be confessed that a journal which contains articles under these four headings, particularly when produced under the guiding genius of Dr. Churchman, would well be one of great interest and importance to all operational research workers. Having said this it is a pity that when one studies the first volume it is seen that most of the articles are exercises in pure mathematics.

The Operations Research Society of America and the Institute of Management Sciences are very fortunate in having a significant proportion of their membership drawn from the universities and research institutes. This undoubtedly means that the basic tools and techniques available to workers and analysts are of high precision and are well documented. This influence is lacking in our own Operational Research Society. Unfortunately, while the university research worker now has these two American journals as vehicles in which he may publish the results of his research, the workers in industrial or military operational research units are not using these journals to publish the results of their practical work.

As can be seen from the above, the result of this is that *Management Science* is out of balance, particularly when viewed against Dr. Churchman's guiding principles. Without a doubt the articles which have appeared are of high calibre in the field of mathematics and should be known and appreciated by operational research practitioners. It is to be hoped that the future development of *Management Science* will bring forward a large number of articles with actual case studies showing the practical results accruing from the applications and techniques of management science as well as demonstrating how much and in what way management science differs from operational research. B. H. P. R.

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OPERATIONAL RESEARCH QUARTERLY

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EDITORIAL NOTES

Spreading the News

TWO QUOTATIONS FROM PAPERS published in this issue are, from the editors' point of view, particularly worthy of remark:

D. G. OWEN, speaking at the March meeting of the Operational Research Society, said that one of the chief lessons that emerged from the discussion on *The Influence of Data Processing Machines on Operations* was that "what is needed above all else is that people should *use* the machines . . . nobody can yet say 'for this type of application you need that type of computer' . . ."

W. N. JESSOP, writing on *Operational Research Methods* concludes a survey of available literature (not unduly complimentary to ourselves), with this remark: ". . . it may well be that we in this country are not lagging behind in the development of operational research as an activity, but our present contribution to operational research as a subject is almost negligible. It is therefore necessary that more work, and of a practical character, should be published in this country, so that impetus may be given to the development of techniques which are truly 'operational research methods' in the sense that they are of widespread service".

While it may be that Mr. Jessop underestimates the value of current British contributions to the practice and methodology of operational research, no one can dispute the justice of his plea

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for prompt publication of results. The policy of the Operational Research Society, both in founding this journal and in urging its members to publish their work in it, and the eagerness of the *Quarterly* to perform this service are well known to our readers. That these stimuli have not been without effect is shown by the fact that the number of pages published has increased in each of the six completed volumes of the *Quarterly*.

There is still, however, a large margin for improvement, and we hope that Mr. Owen's implicit and Mr. Jessop's explicit emphasis on the value of publishing results as promptly and as fully as possible will reinforce the encouragement already given to those in a position to submit papers for publication.

O. R. Q. Subscription Rates

It is with regret that the editors are compelled to announce an increase in the rates of subscription to the *Operational Research Quarterly* to take effect from the end of the current volume. From the first issue of 1957 onwards, the new rates will be:

	British Commonwealth	*Other Countries*
Surface mail	20*s.*	30*s.* ($4)
Air mail	40*s.*	60*s.* ($8)
Single copies (*surface mail*)	6*s.*	8*s.* ($1·20)

There has been no previous increase in the subscription rate in British Commonwealth countries since our foundation in 1950, and only one increase in non-Commonwealth countries, to absorb higher postal charges. During these seven years we have grown in size from 72 pages in 1950 to 176 in 1955, an increase of 144 per cent. But higher costs have been counterbalanced by a steadily growing circulation, which now stands at just under 1,000 paid subscriptions.

The latest increases in printing costs and postal rates, however, leave us no choice but to increase our rates as noted above. At the same time we have revised our arrangements for supplying back issues to ensure that even those (the majority) which are now out-of-print will be available to purchasers wishing to complete their files. Such issues are now available in photographic reproduction from a London firm, to whom we shall be pleased to refer enquirers.

The change in subscription rates makes no difference to the arrangement whereby members of the Operational Research Society receive copies as part of the service to subscriptions to the Society.

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THE QUANTIFICATION OF NEW INVENTIONS

by

THE EARL OF HALSBURY*

FOR A NEW INVENTION TO BE spontaneously adopted it must be simultaneously advantageous to four interests: the financier; the producer; the distributor and the consumer. If the interests of all four of these parties can be simultaneously engaged, adoption of an invention will be spontaneous. If not, it will languish until someone resolutely overcomes the barrier represented by a missing factor. My work at N.R.D.C. has taught me to approach the problem of quantification in these terms. Assessment on behalf of the producer and consumer appears much easier than assessment on behalf of the financier and the distributor.

Finance

Regarding the development of an invention as an investment, how does it compare with other forms of investment? This question is a tricky one and the answer needs some care. It is profitable to consider, for comparison, investment in a timber plantation. First it should be noted that two entirely different situations fall for consideration. Planting may proceed in balance with cutting on mature forest land, or planting may be on virgin or de-afforested land so that no yield can be expected for some thirty years. The two situations compare with feeding material into one end of a pipe line from which delivery is being effected continuously at the other end, and filling an empty pipe before delivery can commence. Reverting to timber, investment on planting virgin land is comparable with investment in planting mature forest land for one reason only: there is a market price for growing timber; stands of young timber can be sold *in situ*, not for cutting but as an investment. Thus, while no return may accrue for thirty years or more following the planting of virgin land, a single investor need not necessarily immobilize his capital for so long as 30 years. Thirty investors may carry the plantation for one year apiece, each receiving an appropriate capital appreciation on re-selling his last year's purchase.

It is interesting to compare the financing of inventions with the financing of timber. Both are long term investments. Inventions which fall into the pattern of evolutionary advance characteristic of a mature industry compare with planting mature forest land so as to keep pace with cutting. Inventions of a revolutionary character which

* Managing Director, National Research Development Corporation.

cut across the established industrial pattern, requiring a new beginning to be made in an unexpected direction, compare with planting a forest on virgin land. The comparison breaks down however on noting that there is no market price for a partly developed invention as there is for partly grown timber.

Let us suppose that the return on money invested yields x per cent, such that £100 invested now will become £200 if invested at compound interest for 15 years. It is clear that £100 invested now on a basis of "no return" until the expiry of 15 years must thereafter be thought of as £200. If it is to yield x per cent on its notional value of £200, it must yield $2x$ per cent on its original value of £100. If the period of no return were 30 instead of 15 years, the notional value of the £100 should thereafter be £400 and a yield of x per cent on this sum would need to be equated to $4x$ per cent on the original £100. As x is about 5 per cent (4·6 per cent to be exact) it follows that if the investment were a perfectly safe one, it would require to show about 20 per cent return after the lapse of 30 years with no return in the interim.

The development of an invention is not however a safe investment. If one attempts to scale up returns of 20 per cent by likely values of risk factors assumed to be appropriate to the case in point, one rapidly reaches figures for yield so high that their ultimate attainment cannot be predicted with any sort of certainty. These considerations lead one to the conclusion that *no realistically postulatable yield in a speculative investment without an intermediate market price is adequate remuneration for waiting* 30 *years before realization.*

If this conclusion is correct, it is understandable why Sir Frank Whittle's gas turbine found little financial support. Whittle first put pencil to paper in 1930. At the present moment (1956) only one aircraft, the Viscount, is flying commercially with a gas turbine. By 1959 full commercialization will have been reached with the Viscount, Britannia, and Comet IV manufactured in England and the Electra, D.C.8, and Boeing 707 in the United States. All these will draw heavily but freely on techniques and developments paid for by defence contracts so that their notional cost of development will contain a large concealed subsidy. Twenty-nine years will have passed since Whittle first put pencil to paper. Is it to be wondered at that only the slenderest support was forthcoming in the early stages? I conclude that *the development of revolutionary inventions on an intrinsically long time-scale is not financially attractive.*

It follows as a consequence that *failure of such an invention to attact finance cannot be adduced as an argument against its merit.*

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Production

Sensu stricto. Production lends itself to diversification more readily than other factors. Production difficulties are rarely an insuperable barrier to the manufacture of something new. A possible reason for this is that the persons concerned are trained on a professional basis. Any engineer should in principle be capable of manufacturing any engineering product of which a working prototype can be demonstrated. So far from being a barrier to production, diversification is positively fashionable in modern industry. A productive organization working with professional staff and standard equipment (machine tools, etc.) can expand without undue difficulty. It is probably the most elastic factor of all those involved.

Development. It will be convenient to couple development with production. It is a much less elastic factor. Development is usually by a team. A team can only tackle one task at a time. It somewhat resembles a colonial organism which opens its mouth once every five or ten years to swallow whatever attractive morsel lies within range. Thereafter it is occupied with digestion and will react with indifference to further food supplies until it has digested its previous meal.

In human terms, directors of research and development cannot be endlessly dropping what they started a year ago to tackle something new. Development cannot be conducted on a slapdash basis, on the one hand, or suddenly expanded by engaging staff to fill in the gaps in a skeleton structure, on the other. A team cannot expand its output tenfold by installing ten microscopes where it originally possessed but one. The limiting factor is not with the number of microscopes but the brains of the microscopist pondering over what his instrument discloses. Ten detectives will not trace a crime ten times as fast as one if they are all working on the same clues.

Distribution

This may be the least elastic of the interests which have to be satisfied. I personally misjudged this factor in developing the Ricardo light steam engine for India and other under-industrialized countries. The project to make a small live-on-the-land prime-mover was basically well conceived. There is a real demand for such an engine. Production lies in a well understood field; no revolutionary innovations are involved on the development front; the steam engine designed by Sir Harry Ricardo is one of many quite orthodox possible approaches all of which could be foreseen to be satisfactory; his highly ingenious design of boiler was the real clue to the development problem and this proved surprisingly trouble-free from the start.

Market research indicated that sales at a remunerative price could be substantial; production finance ought not to have been difficult to obtain. The project however has temporarily broken down over distribution. No one wants currently to tackle the problem of marketing a new prime mover in India because of the political uncertainty. It transpires finally that those who are keenest to make the engine have not got the distribution facilities; those who are keenest to distribute it are not manufacturers; and those who have both the production and distribution facilities are not eager to commit themselves still further to the Indian market in difficult and uncertain circumstances. Distribution is not carried out by professionally qualified people of an overall level of competence guaranteed by a degree or its equivalent. It is basically true that any qualified engineer can do what any other qualified engineer can do. It is not true that any salesman can do what any other salesman can do. A sales organization is built up over the years by individuals with a flair for their work, having experience of their territory and a knowledge of the people with whom they deal. They cannot readily turn their hands from selling swords to selling ploughshares just because the engineers can beat the one into the other with a drop-forge.

So that, when one hears that diversification is fashionable in industry, it must be qualified to imply "diversification in production within the same pattern of distribution".

Consumer

Domestic. Gauging the consumer's interest in a revolutionary invention is difficult; few people can have foreseen the part played in modern life by the photograph, the motion picture, television and the gramophone. It appears to be generally true that one should not take counsel of one's doubts and that *the consumer is always interested in a revolutionary invention.*

It is otherwise with non-revolutionary inventions. A recent example of the exploitation of one of these is the Knitmaster, a household knitting machine. So far as I know it does not involve principles differing substantially from those of the Ring-frame knitting machine of A.D. 1589, though it is linear. Something very similar to the Knitmaster could have been marketed at any time in the last century. Someone has recently decided to push the sales of this device, with evident though possibly transient success. Will the Knitmaster prove as ubiquitous a feature of domestic life as the sewing machine in 50 years' time, or will it prove a flash in the pan? If it becomes a permanent feature of domestic life, could it have been successfully introduced fifty years ago? Is it basically less essential

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than a sewing machine, so that its introduction *en masse* had to wait until every household had both a sewing machine and a margin of purchasing power available for investment in a knitting machine as well?

The household refrigerator and deep-freeze are other examples in this class. No basically new inventions were generally involved (the hydrogen-ammonia system is an obvious exception to this statement) as opposed to formulation of household designs practical to install and cheap to fabricate. These developments ultimately serve distribution patterns. It is difficult to foresee exactly where the margin of domestic purchasing power in an advancing economy will next evoke a pattern of supply. The gay, rationalized layout of a modern American kitchen in stainless steel, pressed metal work and plastic will be at least one copious field for exploitation by designers in the years to come. When Mr. Everyman has an automobile, radiogram, television set, vacuum cleaner, refrigerator and deep-freeze, rationalized kitchen, washing machine, drying machine, washing-up machine, sewing machine, knitting machine, tape recorder and camera, where will he seek to expend his next £100 of freely expendable income? It would take a genius to answer this question adequately.

Industrial. There is one big difference between the industrial and domestic consumer. The former buys, not from reasons of choice, but of cost and economy. He knows the figure at which a new facility would be acceptable to him and no amount of advertising or high pressure salesmanship is likely to affect the judgment of the engineer and the cost accountant, working as a team concerned to quantify the merit of the goods they buy. Moreover, what is true of one industrial buyer will be broadly true of another in the same class. In a sense then, the price at which an industrial consumer will be interested in a new process or product and the quantity likely to be required at that price, is much easier to gauge than the corresponding factors relating to the domestic consumer. The problem is further simplified by the fact that there are much fewer of them. Again, however, it is necessary to point out that the more revolutionary a novelty is, the less easy are these assessments to make.

Our assessments are usually made by extrapolating not too far away from a corpus of standard practice. Once well clear of this corpus, however, a large intuitive element enters into any judgment involved. Inventions may be more far-reaching in their secondary than their primary effects.

It is easy to ascertain, for example, that the cost of wiring a radio or television set is a not very considerable part of the whole. If it could be performed for nothing, the price of the set would not be very materially changed. On this basis the advantages of employing

printed circuitry could not be very considerable. The cost of inspection, however, may be a multiple of the cost of wiring. In the case of a complicated assembly such as an electronic computer, inspection may cost as much as four times the cost of wiring. How much inspection will be saved by printed circuit methods of assembly? This sort of question cannot be answered on the small scale. What about automatic assembly and dip soldering? They would be possible with printed circuits, but scarcely otherwise. What of the reaction on overall designs of automatic assembly?

It is obvious that these questions are too difficult to answer in terms of advance calculation and that inventions relating to printed circuitry must be assessed, not in terms of the immediate savings they bring about, but of the fabrication and design of opportunities which they open up. These however can never be opened up until *after* printed circuits have been adopted. *First* there must be printed circuits. Only then can the opportunities they open up be exploited. It is where assessments of this kind are involved that the accountant's facts are at their least, and the engineer's intuition at its most important.

Summary

The assessment of inventions may be thought of in terms of four factors, each to some extent capable of quantification. An isolated long term revolutionary invention cutting across the main evolutionary pattern in its field will always be an unattractive financial investment.

Development facilities fall due for redeployment only sporadically and unless free facilities coincide in time with the epoch when an invention requires development, industrial sponsorship will not occur spontaneously even if the invention is of the evolutionary rather than the revolutionary type. Production facilities are the least difficult factor of all those requiring satisfaction.

Distribution facilities are the least elastic of the factors. If the pattern of production and distribution indicated as desirable does not pre-exist in one establishment, development will probably be inhibited on the distributive side.

The domestic consumer is, as a matter of history, always spontaneously interested in a revolutionary novelty. His interest in evolutionary design is more difficult to assess. The industrial consumer is affected in exactly the opposite way. His interest in evolutionary designs can be readily gauged. His interest in revolutionary novelties, however, is apt to be at fault due to under-discrimination between the primary and secondary effects of an invention.

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The adoption of an invention by industry is best quantified in terms of the concept of spontaneity. Inventions should be thought of less as 'good' or 'bad' than as conforming to an established evolutionary pattern or cutting across it. Industrial adoption should be characterized less as 'progressive' or 'backward' than as spontaneous or inhibited. Inventions cutting across the established evolutionary pattern will tend to be inhibited; those conforming with it will be received by industry with a high degree of spontaneity.

This paper was given to the Operational Research Society *in London on 11 January, 1956.*

OPERATIONAL RESEARCH AND COMPUTERS

AN OPEN MEETING OF THE SOCIETY to discuss *The Influence of Data Processing Machines on Operations* was held at the rooms of the Royal Society, London, on 9 March, SIR CHARLES GOODEVE in the Chair.

The Chairman suggested that the introduction of machines which would process data of a type and at a speed inconceivable except to a few people a generation ago, marked a step comparable with the discovery of the uses of geometry by the Greeks and Egyptians, and the introduction of the nought into arithmetic by the early Hindus.

The discussion would largely turn on the effect of these machines and of the new advance in mathematics, or more properly in the equipment of mathematics. It was a much bigger step than that of the nought of the Indian-Arabic system of counting, and its effect would probably be correspondingly greater. Its impact on civilization would be on the operations of society, which was what made up civilization, and it was that which was perhaps to be the main theme of the discussion.

DR. B. V. BOWDEN (Manchester College of Technology) traced the history of computing machines, taking a Chinese abacus as his starting point. His illustrations included Pascal's computing machine, a Leibnitz machine demonstrated to the Royal Society in 1678, and machines made by Babbage. Dr. Bowden paid tribute to Babbage's genius. Babbage had appreciated the facts that: "if you can make the machine carry out the same series of operations time after time and day after day, although on different numbers, and, furthermore, it can steer itself according to the result that it gets and vary the

exact forms of calculation made, then only can it come into its own as being able to do arithmetic faster than a human being can.

"It is astonishing to find", continued Dr. Bowden "that for so long after computing machines were being built in England and America these points were not grasped. No machine built before 1942 had in it these abilities which Babbage realized to be necessary."

After a brief discussion of the fact that the rate of a machine is limited by the power of the mathematician to instruct it, Dr. Bowden gave some examples of application to Operational Research.

"The Douglas Aircraft Company some years ago", he said, "was confronted with the fact that it had to accept several government contracts from a list supplied to it. The company was able to carry out a survey of the impact of all the possible contracts on its works, knowing the sort of demand that each would make on draughtsmen, engineers, the machine shops, and so on. Over a week-end it was able to carry out a calculation which involved 500,000 calculation sums, and came to a definite decision as to which set of contracts it was to its advantage to accept."

Having taken on the job of designing a particular jet aircraft, the company analysed the properties of 536 separate designs of aircraft —postulating different combinations of speed, range, shape of wings, and so on. This represented an amount of work in arithmetic which it would have been quite inconceivable to do in any other way. The total amount of work involved in designing an aircraft is very large—seven million man-hours—and the D.C.A. jet would never have been built had not an analysis of that kind been possible in advance.

This indicated that it was possible to do numerically the kind of experiment for which wind tunnels and models would otherwise be needed. It was this kind of numerical approach which was the fundamental object of operational research in its widest aspects.

"The economy of the country as a whole cannot be studied by doing experiments upon it", continued Dr. Bowden, "but one can analyse its behaviour in certain circumstances if one has a sufficient power of analysis. Some parts of the analyses now made depend on calculations, others on hunches, and others on deductions from extraordinary, irregular and unestablished facts.

"In about 1833 Babbage did a survey of the price indices of goods available and for sale in Liverpool. He said that economics in general was vitiated as a subject because of the lack of adequate data of that sort—the Board of Trade has been coping with the problem ever since. Babbage said that it should never be feared that

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erroneous deductions might be made from recorded facts because 'the errors which arose from the absence of facts were far more numerous and far more terrible than those which resulted from misreading of true data'."

The kind of project for which Dr. Bowden suggested the machines could be valuable, was analysing the census of production, which the Board of Trade at present failed to analyse in time. The census of 1948 had taken three years to analyse. At least part of the crisis from which we were suffering might be attributed to the fact that the Chancellor based his April budget on calculations on information already out of date, and that many of the remedies which he applied aggravated a situation which had changed since the initial analysis.

It was fair to draw an analogy between the business conduct of our own Government and the general problem of coping with a servo-system. Many engineers who have coped with the problem of stabilizing a servo-system knew that if data was unreliable and out of date any attempt to stabilize the system would make it worse rather than better. A good deal of the difficulty experienced by people in running our country could be attributed to the same sort of mathematical difficulties which have been analysed by servo engineers. . . .

Machines capable of doing calculations 400 or 500 times as fast as a man could make it possible to bring into the whole range of commerce and Government, calculations of the sort of order which classical economists had been fond of postulating but which had never been achieved in practice. Operational research might become wholly dependent on the possibility of doing calculations on the enormous scales which these machines made possible.

"I believe", Dr. Bowden concluded, "that one can now hope with reasonable confidence to achieve that understanding of markets, that understanding of processes of business and that understanding of Government policy which has been hoped for so often and so many times postulated but has never, in fact, been achieved."

DR. S. H. HOLLINGDALE (R.A.E., Farnborough) spoke on *Getting Data into the Computer*, and said that he would confine himself to some remarks on work at Farnborough.

Aeronautical research generated vast quantities of experimental data. Whether the experiment was conducted in a wind tunnel, in an aircraft in flight, on a structural test specimen, on a guided missile either observed from the ground or sending signals back to the ground, the immediate product was a mass of experimental records which might appear in a variety of forms.

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Most of the raw data had to be processed in some way. This might mean no more than applying simple calibration corrections, but it usually entailed more substantial computing. Finally, the results were often wanted in graphical form, and so even the computing was not the end of the story.

Most of this kind of work was done by junior staff who were loosely described as "computers". A survey at Farnborough a few years ago revealed that computing proper ("doing sums") only occupied about half the time of the so called computing staff. The other half was spent in transcribing data from one form to another. It was hoped that the arrival of the high speed electronic computer would largely take care of the computing half of the business; it remained to do something about the other half.

Dr. Hollingdale emphasized the necessity of regarding the transcribing equipment and the computing equipment as one unified whole. At Farnborough the problem was the conversion of the raw data into a form suitable for feeding into DEUCE, which takes in its data in the form of Hollerith cards punched with patterns of holes. He discussed two cases: wind tunnel measurements and aircraft flight trials.

Wind Tunnel Problems

"The old procedure", said Dr. Hollingdale, "was to photograph the dials of the various instruments. The photographs, on ciné film, had to be developed and printed. They were then read by one operator, while a second one wrote down the figures. Computing was done on desk calculating machines, and the final results plotted. With this system there was a delay of about six weeks between completing the test and delivering the final answers—half the time being spent in actual computing and half in transcribing the data.

"In the scheme we have now adopted for our large wind tunnels, all readings are first converted into shaft rotations by means of null-reading instruments balanced by electrical servos. While in this form, the readings can be displayed on counters or dials, or, by means of lead screws, be made to draw graphs.

"The next step is to convert the shaft rotations into electrical signals in digital form—what we call 'digitizing'. The final stage is to feed these signals, after some de-coding, to a Hollerith card punch, which prepares a pack of cards in a form suitable for direct input to DEUCE.

"The tunnel operators also want a running record of the measured quantities to guide them in planning the progress of the experiment. For this reason, we have arranged matters so that

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the digital signals not only operate a Hollerith card punch, but also an electric typewriter.

"The whole process up to this stage is fully automatic. The packs of punched cards are then brought to DEUCE, and the necessary computing is carried out by means of a few standard programmes which are kept for the purpose. DEUCE punches out a further pack of Hollerith cards containing the computed answers. These cards are carried into the next room and their contents printed by a line-at-a-time printing device attached to the standard Hollerith tabulator.

"One of the crucial operations in most data processing systems of the kind I am describing is that of converting some physical magnitude—in our case a shaft rotation—into coded digital form; in particular, into trains of electrical pulses along a number of wires.

"The shaft digitizer we use in our wind tunnel equipment consists of a copper disc which embodies a coded circular scale. On the surface of the disc are a number of annular bands, each band being divided into insulating and conducting segments. The copper disc is pinned to the shaft whose rotation is to be digitized. A set of steel balls, held in an assembly which does not rotate, is sprung against the coded disc, each ball being connected to a separate output wire. To fix ideas, let us consider a coded scale designed to read the rotation of a shaft to 1/1000th of a turn. This was, in fact, the requirement given to us by the tunnel people. Then for each of the 1000 angular positions, the coded disc presents a unique arrangement of conducting and insulating segments to the set of balls, which we may think of as arranged along a radius. If a ball is in contact with a conducting segment, current flows along the corresponding output wire; if not, then no current flows. If we identify current flowing with a '1' and no current with a '0', we have achieved a coded binary digital representation of the angle through which the shaft has rotated.

"Coded commutators of this type have a wide field of application—to the recording of process or inspection data, to the control of mechanical plant or machine tools, and so forth. We have provided a number of different scales to meet particular needs. An alternative technique is to use coded discs consisting not of insulating and conducting segments, but of transparent and opaque segments which are read photo-electrically. Resolutions of a few seconds of arc should be possible in this way.

"We are also developing multi-turn digitizers to encode the positions of shafts which can rotate through more than 360°.

"In order to minimize errors due to small mechanical misalignments in the equipment, the commutator code should be such

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that the binary representations of two adjacent integers, P and $P+1$, differ in one and only one binary digit. This must be true for all values of P, including for instance such awkward cases as $P=99$ and $P+1=100$. Codes with this property are known as *CP* codes. I will leave it as *CP*, as no-one is sure what the letters are supposed to mean; cyclic progressive, cyclic permuted and continuous progressive all have their supporters.

"The second point to bear in mind when choosing the code is that the process of de-coding back to the ordinary decimal representation—which is needed, for instance, for the typewritten record—should be as simple as possible."

Discussing the economics of this system, Dr. Hollingdale gave some typical figures for one of the supersonic tunnels at Farnborough.

With fully automatic recording and plotting, the necessary staff was cut from 7 to 3 while the tunnel was running, when the final results were plotted and computed, man-hour demands would be cut by a factor of about 40, to about one man-hour per tunnel-hour instead of 20—the factor being 40 instead of 20, because automatic recording enabled the measurements to be made twice as fast.

At present computing was done automatically on DEUCE, but plotting was still manual, and took about four man-hours per tunnel-hour. The improvement factor was thus about 8.

New difficulties were now arising, however. At one time a limiting factor had been the shortage of junior staff for recording, computing and plotting. Now it was the shortage of more senior scientists, who had to digest and interpret the results, write the reports and plan the future work. Another limiting factor which had been aggravated by the new procedures was the scarcity of aerodynamic models to put into the tunnels. These models had to be made to very high precision, and with present techniques it might take some thousands of man-hours to make one of them. This problem was being tackled separately by the introduction of automatic control techniques at the manufacture and inspection stages.

Flight Tests

The experimental data obtained from aircraft flight tests were usually recorded in analogue form; for instance, as visible traces on rolls of paper. The ordinates had usually to be read at successive instants of time—time being read along the length of the traces. Once again, the basic problem was to convert these readings to a form in which they could be supplied to an electronic computer.

For this purpose a trace reader had been developed at Farnborough. The operator selected an instant of time by moving the

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DECIMAL	PLAIN BINARY	REFLECTED BINARY	R. A. E. BINARY
0	0000	0000	0101
1	0001	0001	0001
2	0010	0011	0011
3	0011	0010	0010
4	0100	0110	0110
5	0101	0111	1110
6	0110	0101	1010
7	0111	0100	1011
8	1000	1100	1001
9	1001	1101	1101

PURE DECIMAL	C.P. DECIMAL	R.A.E. CODE
08	08	0101, 1001
09	09	0101, 1100
10	19	0001, 1100
11	18	0001, 1001
518	581	1110, 1001, 0001
519	580	1110, 1001, 0101
520	570	1110, 1011, 0101
521	571	1110, 1011, 0001
698	691	1010, 1101, 0001
699	690	1010, 1101, 0101
700	790	1011, 1101, 0101
701	791	1011, 1101, 0001

FIGURE 1. *Coding at R.A.E. Above, binary coding of the decimal numerals. Below, examples of binary-within-decade codes. The number, in ordinary decimals, is first converted to "CP" decimal code as shown in column 2. Each of the resulting decimal digits is thus coded as 4 binary digits in accordance with the scheme shown in the upper table. Only one binary digit changes in transfer from any number to its neighbour.*

paper longitudinally by hand, and then moved the control arm to set a cursor on the required trace. In doing so he also moved a set of reading brushes over a coded scale. This scale was similar in principle to those already described but was linear instead of circular. By depressing a foot-switch the operator caused electrical pulses corresponding to the reading of the brushes to pass first to a decoder—which converted from the R.A.E. code to ordinary decimal—and then on to a Hollerith card punch and an electric typewriter, as in the wind tunnel case.

This trace reading equipment had proved popular, and operators quickly settled down to a rate of about 1,000 readings per hour.

The system was semi-automatic, and a fully automatic system for processing flight test data was also being developed. In Dr. Hollingdale's opinion, the right approach was to digitize the data as early as possible.

"In our scheme", said Dr. Hollingdale, "each aircraft instrument is fitted with a slave unit which can follow the pointer of the instrument. Each slave carries a coded circular scale of the kind I have already described. The scales attached to the various instruments are read in turn by a digital recorder in the aircraft. This device records the information in punched tape form. Up to 12 holes may be punched across a line of tape corresponding to a reading of three decimal digits or a resolution of one part in a thousand. Twenty-five instrument readings can be recorded per second in this way. The tape is then read on the ground by a simple device. Once again pulses are emitted which operate both a Hollerith card punch and an electric typewriter.

"The common feature of each of the three systems I have described—two fully automatic and one semi-automatic—is that the raw data, which appears either as an angular rotation or a linear displacement, is converted into a coded digital form. Once this has been done, it is a relatively simple matter to record the information automatically in a form suitable for direct input to an electronic computer, and also to provide a typed or graphical record for immediate inspection."

D. G. OWEN (British Iron and Steel Research Association) said that computers were used for speed, accuracy, and "obedience" or ease of programming. There were three broad classes of use: (1) research; (2) commerce and business communications; (3) productive industry.

In research one of the main uses to which digital computers were put was for statistical analysis, of which a good deal more was probably needed.

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The second main use was for mathematics, such as that "with which the research team is asked to deal when the physicists or the engineers produce some ghastly equation for some property of a material and they want it solved. The calculations which can result from that in order to obtain a numerical solution can definitely require a computer".

Thirdly, there was the processing of automatically recorded data, such as that referred to by Dr. Bowden.

Finally, and possibly ultimately the most important use in research was the employment of computers to simulate experiments to forecast the results of reorganizing a process or situation in some large or small respect. There were, for example, many queueing problems in industry. Their formulation in mathematical terms, was often extremely difficult, but simulation by Monte Carlo or other methods offered a prospect of an answer rather more quickly.

Mr. Owen suggested that three lessons could be learnt from these considerations.

Methods, he thought, should be formalized. Many of the simpler calculations carried out very many times in the course of research, such as the formation of frequency distributions and characterizing distributions, fitting curves, and so on, could be formalized. It would then be possible to build up computer programmes which would free the research worker from an appreciable part of the donkey work.

Secondly, Mr. Owen advocated automatic recording of the data required for research as a by-product of the process. Alternatively, one could introduce simultaneous transcription of data into both written form and punched tape form. "A great deal of data comes into London and other centres of research, commerce and business", he said, "in the form of typed and completed questionnaires and forms. If this data has at one time or another been typed, there is no reason at all why the operation of typing it should not simultaneously put it on to cards or tape. Opportunities for doing that exist on all sides, and they need to be taken."

Thirdly, Mr. Owen suggested that the introduction of computers afforded an opportunity for surveying the field of research—for looking at the "rather larger problems of which we have been frightened for years but which we might now seriously tackle again. Some of these vistas are opened up by the prospects of doing by means of Monte Carlo calculations problems which, mathematically, have always proved intractable, and there may be others".

In commerce, Mr. Owen suggested that opportunities existed for the use of computers in insurance and banking, which were

"clearly organizations which necessarily do a fair amount of arithmetic".

He also mentioned the possibility of an electronic Stock Exchange, and possible use in the mail order businesses, the Inland Revenue and the Ministry of National Insurance, which he knew were interested in the subject.

Mr. Owen suggested that the use of computing machines in ways such as these might greatly widen the range of services supplied by commerce. If this were the case, there would be an increased need for people capable of understanding and operating the machines. Programmers, he thought, need not necessarily be mathematicians.

In industry, the most notable use of computers so far was in aircraft design. Of other uses he suggested the most important or urgent were probably production control and stores control. Computing machines offered an opportunity of linking up the three essential phases of productive industry: planning what is to be produced; producing it; progressing, cost analysis and similar functions.

After production control and stores control Mr. Owen foresaw the application of computers to all production and cost records, including the calculation of wages, such as was already being done by J. Lyons & Co. Ltd.

Eventually, we might look forward to actual process control, which already existed in certain respects. Mr. Owen remarked that already milling and drilling machines, for example, could be controlled by magnetic or punched tape prepared by a computer from product specifications or drawings. This was an application not dependent on a huge steady production of a product. In fact, the more varied the production of the machines, the more useful the automatic control of them could become, and this might also be true of production schedules.

Discussion

The Chairman said that Mr. Owen had made him think that it might be time to start teaching children at school to count in the binary system. As an experiment, he and a friend of his had tried it on some children. It was astonishing how quickly a boy or girl under 10 years of age could learn to count and multiply in the binary system, but it took a long time to teach an adult how to do it.

J. A. Gosden (J. Lyons & Co.) said that his firm's wages calculations were familiar to most people, and he would not spend any time upon them; they were the first larger scale job that Lyons put on the computer in order to satisfy themselves that something could be done.

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They had then turned their attention to the daily task of teashop ordering. They wanted a method that was simple and easy for the teashop manageresses to carry out, and they wanted more details and facts so that they could see what could be done to improve profit margins and service to the public.

The requirement from the manageress's point of view was to order goods for delivery as little in advance as possible, ideally the previous day. Previously she had had to fill in afresh each day numerous forms of different shapes and sizes. It was realized that, in fact, the manageress's indent was always a revision of a previous estimate. So now the central office keeps a record of the manageress's order estimated in advance, and every day the manageress revises it, altering it only in respect of the requirements which have changed. If the manageress is lazy, or is rushed, or falls down dead the order goes through as originally estimated.

The system is very easy to operate. Each manageress is rung up daily by Cadby Hall at a set time, after she has reviewed her advance order. She is asked to quote for any item for which she wants to make a change, the item number and the new amount required. This is all the new information that needs to be given to Leo to enable it to work out the full revised order.

The information is punched straight on to a card by the girl at the telephone, who then calls it back to the manageress. The cards are then taken into the computing room next door and put in order and processed through the computer. This produces the packing lists, which are taken downstairs to where the packing takes place for the first delivery in the morning. Concurrently with this process further statistics are produced for the use of the management.

The manageresses have found it a wonderful system. They now have little clerical work to do whereas previously they had had to spend more time than enough with the big book of order forms. Accounting, too, is much simpler. While the packing lists are being printed, totals are being built up and at the end of the computer run a reconciliation is provided which proves that everything has been done correctly. The computer is able to carry out classical double-entry book-keeping as it goes along since it charges the teashop which is to receive the goods and credits the section of the despatch which provides them.

The system produces the statistics which the management require to control the teashops. There is a report each week on significant changes. The computer does not produce the full volume of statistics which are possible. The aim is to select the information which is most significant, and on which action appears to be

necessary. Figures for the whole country, for example, can be issued to the management, pinpointing for any aspect the divisions most concerned. Similarly statistics for each division can be issued to the divisional manager, pin-pointing the sections most concerned, and statistics for each section can be issued to each section supervisor, pin-pointing the individual performances of the teashops where these were worthy of note.

There is also something of a forecast of sales by reason of the manageresses having had to make advance estimates. Further, it is possible to obtain more in the way of selected statistics when required —for example separate facts relating to particular lines. Another feature of the job is the possibility of varying the quantity of goods to be despatched because of over-riding management or production considerations. At the very last minute one item could be substituted for another or a percentage increase or decrease could be applied to different grades of commodities because of sudden changes in weather.

Discussing criteria of plausibility, Mr. Gosden said that his firm had found it to be largely a matter of experience.

On the need for mathematicians as programmers, Mr. Gosden said that Lyons had now trained a number of programmers, and a significant proportion of them were non-mathematicians. Using a carefully developed training course they had made suitable arts graduates into programmers, and others who were not graduates at all. It was the type of mind and not the type of academic training that was important.

There was also the matter of simulation. One could think of an automatic computer of that kind as a variable model. Often a lot of operational research was done by means of models, and sometimes they were expensive and difficult to build. Quite often, he suggested, the model could be replaced by a programme in an electronic computer.

S. Wood (G.P.O.) said that the Post Office was proposing to use an electronic machine for wages calculations for all the staff in London, who numbered 112,000 and were scattered over numerous offices. The machine would be in a centralized office. The Post Office had not overlooked the prospect of putting the information into the machine by long-distance control and getting the results out of it by the same means. He would not like to say that the Post Office had solved the problem, but it saw the possibility and was working on it. He emphasized that these remarks were quite unofficial.

Mr. Newman (Ministry of Pensions and National Insurance) said that the use of computing machines for his Ministry was being

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considered by the National Physical Laboratory. The main connection of his Ministry with the problem was that it was acting as the guinea-pig, and it would be better that conclusions should be reported by persons from the N.P.L. rather than from the Ministry.

The Ministry had, however, made some use of high speed electronic computing machines. One example related to the payment of benefit for sickness, injury, maternity and unemployment, and the problem of forecasting the results of changes in the very complicated regulations that had been laid down governing the amount of benefit that was paid. The ordinary methods of estimating the effects of such changes in regulations could not be applied. They were forced to consider the possibility of taking a sample of benefit experience and ascertaining what would have happened and what benefits would have been paid if the regulations had not been as they were at the given time. The amount of staff needed to do the clerical work involved would have been prohibitive. Nor was it easily possible to use ordinary punched card machines. The National Physical Laboratory suggested that the problem could be tackled by an electronic machine, and they did obtain some information. But the problem proved to be much more difficult than they had at first thought, and it was necessary to cut down the information sought, in order to get it out in the time available on the machine and for the amount of money that they were prepared to spend on it.

The interesting point about it was not so much the problem itself but the fact that if one had such a machine available one could begin to tackle problems which it had previously been impossible to consider tackling. Without an electronic machine they would have had to guess what would happen if the regulations were changed, and it would have been no more than a guess depending on the intuition and skill of the individual making it, but with the machine it was possible to ascertain what would actually happen as a result of a change in the regulations on the basis of a large sample of past benefit experience.

The speaker had been interested in Dr. Bowden's suggestion that now that such machines existed one could begin to consider other problems with which it had been impossible to deal previously, but he sounded a note of caution. While there were problems which one could now begin to tackle for the first time, they might prove to be far more complicated than they were at first thought to be.

He thought that the introduction into programmes of tests for plausibility was very important, and this contributed materially to the value of using electronic computers. It was not always remembered that it was possible with machines of the punched card type

to introduce tests of plausibility, but they were not always introduced as frequently as they could be.

R. H. TIZARD (N.P.L.) said that at the N.P.L. computational work was carried out for other scientific and engineering organizations. A large proportion of this work was done by electronic digital computer, and this was charged for at the rate of £30 per hour of machine time. The amount of this work, and its value to the customer was indicated by the fact that the receipts for it in the last financial year were £30,000, and in addition the computer had carried out a further £30,000 worth of computation which was not chargeable.

The N.P.L. had joined with the O. and M. Department of the Treasury and with the Ministry of Pensions and National Insurance in forming a group to study the use of high speed computers for clerical work. In the first place the subject of wage accounting at the M.P.N.I. Central Office had been studied, and a report on this would shortly be published by the Stationery Office.

The actual computational work involved in pay-roll accounting was not a large proportion of the whole, which was made up mainly of marshalling and collating data. In assessing any computer application, it was necessary to consider how much of this marshalling was done by the machine, and how much was still carried out clerically.

For the M.P.N.I. payroll study they had aimed at devising a system which was as fully automatic as possible, even though this was not necessarily the most economical. The result was a system in which the entire clerical work was carried out automatically, human beings being required only for handling the machines and for getting the initial data into the form in which the computer could read them.

Although the M.P.N.I. payroll was a simple one in the sense that there was no piecework or bonus system, it had complications such as the incidence of annual salary increments which occurred on dates individual to each employee. In the system the computer would forecast a fortnight in advance that an increment was due, and would notify the establishment division. If authority either for or against the payment of the increment were received a fortnight later, the computer would act accordingly, but if it were not received the increment would be held over and the computer would send a sharp reminder to the establishment division.

The CHAIRMAN spoke of a problem in British Railways, concerned with the recording and control of goods in transit. Letters and parcels forwarded through the ordinary postal system depended entirely on their directions or label to control their movement to their destinations. Similarly, a wagon-load of goods when it was

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handed over from the commercial department of the railways to the operating department, depended entirely on the label on the wagon to ensure that it was finally delivered at its destination. There was no record of the movement of the wagon from one place to another. Wagons were temporarily lost in their thousands every day. They got into a siding, perhaps taken out of a train for repair, and stuck there until the siding became full and somebody did something about it. This caused a large part of the delay in railway operation.

Another problem related to wagons going into marshalling yards. Arrivals were random, and it was not until the marshalling yard started to fill up that it was known what locomotives would have to be ordered up.

With the developments in communications and data processing machines it was now possible, without extravagant cost, to record a wagon into the system, predict an advance schedule of its movements, inform those concerned, record it at various regular points and then compare the recording with the schedule that it should be operating.

The electronic computer could predict the whole of the operations of a marshalling yard so that the locomotives could be ordered in time, keep wagons on the move ahead of time and also deal with the question of accidental loss of wagons. The system would consist of taking in data, predicting futures, and comparing the circumstances at any one time with the prediction and calling attention to discrepancies between the actual and predicted positions. Furthermore, it could provide operational statistics.

In the speaker's view, wagon recording and control was the only way to prevent the extinction of a large part of the railway goods traffic, which would otherwise follow the building of modern roads in this country.

DR. E. KOENIGSBERG (E.M.I. Engineering Development Ltd.) said that the American railroads were already working this type of system. Several of the railroads had card catalogues of train compositions and these were sent over the teletype system to create new cards at the marshalling yards so that they could be re-arranged into new trains. One railroad was using a preformed teletype message form to send advance notice of trains to the marshalling stations and terminals. They stress the "one-shot process", information is recorded once, all other messages are made up from the output tape of previous messages.

The Chesapeake and Ohio Railway has started using large scale computing equipment for control and accounting purposes, and several other railways had ordered large equipment.

B. D. HANKIN (London Transport) said that the railway freight problem appeared to be one of control. Control required both information and an effective means of action. Electronic devices might detect the movement of wagons and computers might assist in sorting the information, but this would be of limited value unless at the same time effective ways of using the information were being developed and practised.

The CHAIRMAN said that any system that was introduced would have to spend most of its time discriminating between items in the masses of information that it received. It would reject information requiring no action, and place only the important facts in front of the people who should do something about the situation.

He had given only a few of the more important aspects of the system. It was possible to envisage the system's rendering the whole of the marshalling yard operations automatic. Information in the form of punched cards or tape would be fed into the existing equipment relating to the automatic switching of the marshalling yard, and all the wagons would automatically be put into their proper sidings. There would be a great saving of time in that it would be able to predict two, four or six hours ahead what the situation would be in a marshalling yard.

In reply to a question by the Chairman on the probable future relative position of analogue and digital computers, DR. HOLLINGDALE said that the two classes of machine were complementary, rather than competitive. The analogue computer was a special purpose machine. It was less accurate (1 to 0·1 per cent) but, in many cases, considerably faster than the digital machine.

At Farnborough, analogue computers—often called simulators —were used a great deal for studies of, for instance, the motion of guided missiles through the sky, where one wants to do the calculation in "real time", that is, in the same time as it would take the missile to do it. With the digital machine, using a step-by-step process, it might take 100 times as long.

The analogue approach was, in general, favoured by engineers as it gave them a direct physical insight into the process being studied. Analogue computers were particularly useful for exploratory work—for surveying the effect of varying a number of parameters. In this way, the field of interest could be narrowed to limited regions deserving of more intensive study.

D. G. OWEN in replying to the discussion said that the question was always whether, as a result of doing the work on a computer, one could do a calculation cheaper than one could do it some other way.

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What seemed to him to emerge from the discussion was that what was needed above all else was that people should *use* the machines. The designers had now got to the stage where they knew how to make the machines in considerable variety, size and design. But nobody could yet say: "For this type of application you need this type of computer". The reason was that the existing machines had not yet been sufficiently used. An important question when selecting a machine was "Is it or is it not difficult to make a small change in a programme once I have it on the machine?" The answer was that in the case of some machines, partly deliberately and partly by accident, more attention had been paid to speed than to the needs of programming, and it might be exceedingly difficult to prepare the programme in the first place, let alone alter it later. It might be that in those cases one would have to pay more attention to making programming easier. It could be done. Presumably that was an example of lessons which would be learned as time went on.

DR. BOWDEN in his reply said that the meeting had been discussing only the application of computing machines in this country. One was apt to overlook the immensely greater efforts which were being made in the United States where a hundred times more money had been spent on the machines than in this country. The firms in the United States which had begun to use the machines had found that they had paid off and they could not imagine what life would be like without them. There was a universal law that almost anything in that line was easier to get into than to get out of.

OPERATIONAL RESEARCH METHODS

What Are They?

by

W. N. JESSOP*

OPERATIONAL RESEARCH IS STILL YOUNG and self-conscious. It is therefore very much concerned about what it is, how it came to be what it is, and what its standing is in comparison with other sciences—indeed, whether it is a science at all. This latter question has probably been settled, at last, to the satisfaction of most of its adherents. It is agreed that it is a science. The introspective mood, however, continues. It has got to be different from any of its predecessors, it has got to have methods of its own. Certain techniques

* Operational Research Department, Courtaulds Ltd.

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have accordingly received a good deal of publicity as examples of true operational research methods. The purpose of this paper is to discuss some of the most popular of these from the standpoint of an individual employed on operational research in a particular industry, to give a personal assessment of their value and limitations, and to refer to other aspects of operational research activity which, perhaps, have not already received the attention that is their due. It is logically impossible to separate this task from a consideration of what operational research is, but any discussion of this more general topic is incidental to the main theme, and inadequate in itself. I may assume, anyway, that my audience has already heard enough of it.

The dominant characteristic of operational research is that it is concerned with situations which apply in a vast number of human activities, from factory departments to large business concerns as a whole, transport and other services, military organizations, etc. There cannot therefore be an organized body of knowledge in the same sense as applies to physics; the subject matter of operational research is too diverse and variable in itself. Furthermore, in practice it is only concerned with particular cases or situations, or in other words, its job is to solve problems that are specific and local. It is, therefore, a natural consequence that what constitutes operational research is not knowledge but know-how. Problem-solving technique is the bond that links operational research workers in diverse fields.

Confronted with a problem a researcher will ultimately reach the stage of thinking about the method which gives the kind of answer he is after—he recalls an analogous problem that was solved in a certain manner or, more likely, he considers the class of problem to which his problem belongs and the sort of technique associated with that class. Operational research considered as a subject rather than as an activity is knowledge of particular situations and classes of situation and their associated methods. In natural science the unifying and synthesizing process takes the shape of the formulation of natural laws; in operational research it takes the form of recognizing analogous situations and developing techniques to meet them. One can then think of progress in operational research in this sense as distinct from the growth of operational research as an activity. Progress, however, must be conceived in terms of use to the operational research community as a whole and not in terms of volumes of abstract formulation. It is part of the purpose of this paper to examine progress in this sense, of techniques applying to a variety of situations.

I propose to make a selection of what appear to me to be the most popular operational research methods—"popular" in the sense

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of most-written or most-talked-about. Most people would agree, I think, that linear programming is an easy winner in this respect. I would hazard a guess that most operational research workers have tried to find a use for this technique. There is every justification for doing so, since the method applies to situations which are at the very heart of business enterprise—to find the levels of variables at our disposal that maximize profit or minimize cost subject to known restrictions. Furthermore, since the use of this method would probably show results in terms of cash there is a further excuse for looking for problems of this description.

The application of linear programming has not been assisted by the published work on the subject: until recently, there was only one fairly readable book on the subject and various papers in which the same examples cropped-up with monotonous regularity. It is a subject so delightful to the pure mathematician that many papers appear to have had their origin in sheer exuberance unsullied by any thought of a factual situation. This has been one obstacle to its application. We should beware, on the other hand, of such over-simplifications as are now beginning to appear in the U.S. whose relationship to linear programming are like the smile to the Cheshire Cat. It is suggested that one reason why comparatively few case histories of the application of linear programming have been published is that the technique often requires a high level of understanding of technical and accounting matters on the part of the operational research worker and some measure of understanding and sympathy on the part of the management side. This illustrates that the user and the practitioner of operational research have got to speak a common language as far as the formulating of a problem in general terms is concerned; the operational research worker can privately translate into a more precise language for his own purposes.

A more formidable difficulty is the nature of the data which have to be used. Data may be unreliable or inappropriate; most members of my audience will be familiar with works information of the sort that was started many years ago to fulfil a particular purpose and which nobody has since thought to question. Often these data have an innocent appearance of reliability, a snare for the unwary. Cost information may not discriminate when it should, or it may class separately items which have no real difference.

I would urge very strongly that no operational research worker should undertake linear programming unless he is prepared, after mastering the technical side of the problem and its associated data, to impose on himself the agony of understanding the cost

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information that he is going to use, which means that he must form a partnership with the accountant. The problems of communication between specialists and availability of appropriate and accurate information would appear to be strong enough reasons for the slow start in practical application of the method. There is also, of course, the difficulty of computation. If the problem is a once-and-for-all problem of policy this is secondary. If a desk machine cannot cope because the problem is too big, then an electronic machine probably can. It is when linear programming situations arise from day-to-day as they may do in production planning, for example, that the problem is more serious. However, it might be shown that the gains accruing to its use would more than offset the cost of a high speed computer. Alternatively some serviceable rules of thumb might be developed from a series of linear programming experiments with a variety of alternatives. To sum up, the computing difficulties would in many cases be met. In spite of the high degree of development of linear programming theory there are situations to which it will not apply simply because of the condition of linearity. I shall be returning to this point later. Other writers have regretted that the restrictive word "linear" has been prefixed instead of (say) "optimum" which would have covered all cases.

I have spoken about the snags of linear programming and I should like to correct any impression of pessimism that may have arisen. I think it is a valuable tool, and will in time be proved to be so. We should, therefore, be patient, recognizing that the problem of its application is one of the gradual penetration and permeation of the operational research outlook.

Although I would say that linear programming deserves the status of an operational research method there are other techniques that have received considerable attention, which, it would seem to me, appear to be of less potential value in industry. The theory of games is one such instance. It acquired something of a reputation in wartime; there are competitive situations in industry; therefore it would seem a reasonable supposition that the technique would find peacetime applications. With the greatest respect to the military operational research people, I would suggest that the situations for which it found application in wartime were simpler than those which obtain in industry. There are such a multitude of variables and constraints entering into a business situation that a pay-off matrix would indeed be hard to construct. Operations with such a matrix, if one could be found, would be attended by a sense of futility when the unknown and unmeasurable factors that it excluded were thought about.

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Symbolic logic is another technique that has its supporters. It is a good thing for an operational research worker to know about logical notation and its manipulations; it is a discipline and may come in useful some day. But if one were devising an operational research course should it be considered an essential? I do not think that there are a sufficient number of examples of its application (I only know of one in fact) to justify it. The same remarks apply to communication theory.

Queueing theory is a very different matter. There are plenty of examples of its usage and the variety of situations coming within its ambit is enormous. Although the mathematical theory is complex and refers to relatively simple situations, it is clear that it is an essential background to the study of an actual queueing problem. Here theory and practice interact to their mutual benefit. The complexity of certain queueing problems has made a mathematical approach difficult. They have been solved in some cases by the Monte Carlo method. This method is essentially solution by a sampling experiment. Statisticians have used the Monte Carlo for many years and so have physicists from the different point of view of circumventing the mathematical difficulties of deterministic problems. The operational research worker's standpoint is the same as the statisticians'; he is concerned with finding distributions, averages, standard errors of (say) waiting time after making assumptions about inputs, servicing distributions, etc. Here is a method of proved value, and not only in queueing problems to my own knowledge. We have found the method valuable, for example, as a basis for intelligent guesswork regarding the failure characteristics of a plant composed of units whose life distribution under other, but comparable circumstances, is known in fair detail. Another example concerns the best deployment of labour to reduce variability in processing time. The method provides plenty of scope for further developments, one of the most promising being the setting-up of special machines for the solution of certain classes of problem. The big disadvantage of the Monte Carlo method is the amount of labour entailed, particularly if it is a distribution that is under investigation, and there may be a number of alternative models to explore. A special machine can sample from the distributions of the model, using its own supply of random digits, the machine being set to take into account "conditional" factors or feed-backs. Information is obtained painlessly in this manner. Such developments are under way.

I have discussed what are most frequently cited as operational research methods. Three of the methods seem to me to have application to a variety of situations. Clearly operational research is

concerned with much more than these. It would be interesting if a survey could be made to find out what methods operational research workers do, in fact, apply. I would hazard a guess that it would be found that much of the work lay in the application of standard statistical techniques. Perhaps this work does not find its way into the literature because it is considered too humdrum by those who could contribute. Some of it may be; much of the real work of operational research lies in the preparatory work, understanding the situation and defining the problem, the subsequent handling of data being of no intrinsic interest. (I would like to mention in passing an excellent paper by M. L. HURNI entitled *Observations on Operations Research* in J.O.R.S.A., August, 1954, in which this essential initial work is discussed in considerable detail.) The placing of emphasis on the mathematical methods and on highly abstract treatments of general situations may deter many from placing their investigations on record. If the situation is one of general occurrence, however, I think this attitude wrong. I shall return to this point later.

As far as my own experience is concerned I can say that 90 per cent of the problems we meet involve the use of statistical techniques. This applies to problems of very varied type. Some of them are specific to the technology of the Company. There are those which are concerned with the study of data arising from accepted processes, what previous speakers to the Society have referred to as "uncontrolled data". I am sure that studies of this sort are a common and important activity so that the perfecting of sampling and regression techniques in this field is one of real interest to operational research workers. We find many problems in which the statistical approach is standard—analysis of variance, regression, probability theory etc.—but there are other problems in which one feels doubts about commonplace statistical models. This is particularly the case where one is dealing with observations in time which may represent an evolutionary process and one has to make assumptions concerning it. Such examples occur in both technical and economic contexts and include some of the most unsatisfactory problems falling to the lot of the operational research worker. We are also, on the other hand, concerned with experiments under production conditions which permit varying degrees of choice in the factors to be varied. Other problems fall into the sphere of sampling, for example, raw materials and products. There are thus a large number of problems concerned with technical control and with effecting improvements in a technological sense. These problems are only indirectly economic.

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The department is, however, concerned with problems of an economic type and these too require the use of statistical methods. It has been concerned with problems of the most economic length of production run, stock control in its relation with sales and production, and what might be called "break even point" problems. I would like to amplify the latter—they are problems concerned with the tailing-off of a production curve. It is where a change-over to a new product is imminent and the production rate is slowing down. A balance point has to be found, taking into account waste, at which a change-over is made, so that the cost per unit of production is minimal. These situations, involving a number of variable factors, tend to be complex so that a direct statistical approach is difficult. The Monte Carlo method is particularly serviceable in such cases. There must be a considerable amount of work of a similar type going on in other industries in which it is statistical methods that play a dominant part. Linear programming situations are by no means the only ones of cost minimization with which operational research is concerned.

I have given a personal appraisal of techniques that have been cited as "operational research methods". Let us now take a look at operational research literature to obtain some idea of what topics are most written about, and in particular, what proportion theoretical papers form of the whole. To our shame it has to be admitted that this modest survey largely consists of a study of American publications. Taking books to start with—there is the admirable book on wartime operational research by Morse and Kimball. This book is very well known and little need be said about it. It was to be expected that its influence would be great and this has certainly been the case, probably to excess. A more recent work is *Operations Research for Management* based on papers by various authors presented at a seminar at the Johns Hopkins University. It is divided into three sections; general, methodology and case histories. The case histories show, as the title of the book would imply, an interest in non-military problems and they are drawn from a very wide field. In the methodology section the favourites game theory, symbolic logic, communication theory, and, of course, linear programming make their appearance. As papers they are excellent, but readers would be more impressed if they had practical counterparts in the case history section. Prentice Hall's *Handbook of Industrial Engineering and Management* contains a section on industrial operations research under the editorship of A. W. Swan. Apart from an introductory essay on organization the section is devoted entirely to case histories of which there are eight. Operational research methods are not

discussed as such, and if there is a common link between papers of very varied character, it is in the use of fairly elementary statistics in five of them.

Consider now the contents of J.O.R.S.A. A division is made of the contents of 11 issues into 3 classifications: "General", "theoretical", and "case history", where the first refers to papers on the function, scope, etc. of operational research, the second to papers on techniques as such, and I have taken "case history" to mean anything in which there is the slightest suspicion of data.

CONTENTS OF J.O.R.S.A. (SIGNED PAPERS)

General	Theoretical	Case History	Total
11	39	19	69
(16·0%)	(56·5%)	(27·5%)	

The proportion of theoretical papers thus more than outweighs those in the other two classes.

A breakdown of the theoretical papers by subject is as follows:

Allocation theory	Game theory	Linear programming	Monte Carlo method	Production inventory techniques
3	2	3	3	5

Queueing and traffic	Statistical	Sub-optimization	Miscellaneous (mathematical)
6	6	2	9

It will be realized that tables of this sort, applying to something that is developing, have to be taken with reservation. It is fair to say that the earlier numbers read like a sequel to "Morse and Kimball", but the influence of non-military inspiration has become more noticeable recently.

The following is an analysis of the "Case History" papers:

Commercial	Linear programming	Military	Production/ inventory	Statistical
2	2	6	2	1

Traffic and queueing	Others
3	3

There is another publication in the U.S. concerned with operational research and that is *Management Science*. It has not been in existence very long but the contents so far strongly resemble those of J.O.R.S.A.

It would be misleading to present a comparable analysis of the British *Operational Research Quarterly*, since the early issues are scarcely more than notes on Operational Research Club meetings, and at present it does not rise above three papers per issue. Reflecting as it does the programmes of the Operational Research Club or

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Society it tended initially to concentrate on papers of the "survey" type, i.e. on applications of operational research in a fairly wide field, although there is a tendency now for papers to be more specialized.

The American papers show what seems to me to be an unhealthy bias towards abstract topics (we have to recognize at the same time that we in this country are in a poor position to be critical). There would seem to have been an assumption that peacetime operational research was going to be an extension of wartime operational research as far as methodology was concerned. That industrial operational research involves different situations, however much we might conjure with words like "decision-making", should now be realized.

The basic "stuff" of operational research are problem-posing situations and the development of operational research as a subject depends on the amassing of such material. There is no such thing as "pure operational research" but a distinction should be made between general and particular problems. General situations which are the sources of operational research methods may present special mathematical difficulties. For example, mention has been made of non-linear programming. One of the ways in which a problem of allocating production to machines can be non-linear is when cost depends not only on what machine produces which product, but on the number of changeovers that a given machine is subject to and on the order in which products are allocated; the production against time curve may also put a premium on long runs which introduces another complicating factor. Such situations must be of fairly general occurrence and deserve intensive study. Considerable scope thus exists for work at an academic level if it can only be kept in touch with genuine practical requirements. This can be partially accomplished by personal contacts, but it would be made easier if there were more pooling of experience of a practical sort. The key to progress then lies to a considerable extent in the published literature. If we are to develop operational research techniques which are of general use in situations recurring in different fields, much more work should be published, particularly of the practical example or case history sort. By pooling information on (say) stock control, in the course of time general features should be recognized and methods of approach of wide application developed. We use the term "case history" which has strong medical associations and what I am proposing is simply the extension of this analogy. Doctors are continuously writing papers about Patient 'A who had such and such curious symptoms, what treatment was given and how the patient responded. Sometimes an accumulation of such evidence

leads to the defining of a new disease in the medical text-books with an account of the measures that have proved efficacious. Unless descriptions of actual problems are given more prominence in the literature, text-books and courses on operational research are liable to become bogus.

What practical steps can be taken in this direction? Obviously we in Britain must publish more papers. We are living at a time of expansion in operational research and there must be much work of interest not finding its way to publication. It may well be that industrial secrecy accounts for part of the reticence, but surely not enough to account for the pathetic trickle of published work from this country. One is tempted to suppose that some practitioners are so unduly impressed by the ill-gotten reputation of certain techniques and the aura of intense intellectuality surrounding the subject, that they feel diffident about the work they are doing and may even doubt whether they are doing operational research at all. Such inhibitions must be overcome. Why should not the *Operational Research Quarterly* have a good half dozen practical examples of operational research in action per issue instead of two or three?

To conclude, it may well be that we in this country are not lagging behind in the development of operational research as an activity, but our present contribution to operational research as a subject is almost negligible. It is therefore necessary that more work and of a practical character, should be published in this country, so that impetus may be given to the development of techniques which are truly operational research methods in the sense that they are of widespread service.

This paper was given to the Operational Research Society *in London on* 16 *Dec.* 1955.

ROAD RESEARCH

THE FOLLOWING FIVE SUMMARIES refer to a selection of the work undertaken by the Road Research Laboratory. They are reproduced by permission of the Director of Road Research, Road Research Laboratory, Harmondsworth, Middlesex, to whom anyone interested should refer for further information.

Estimated Effect of Projected Road Works in Reducing Traffic Delays in Central London

RN/2415/RFN

Results obtained during a survey of traffic on more than 30 miles of main road in Central London have been used to estimate

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the effect on delay of some road improvements proposed in the *County of London Development Plan*, 1951.

The note concludes that if the improvements were carried out now there might be an increase of about one mile an hour in the present average journey speed of 11 miles an hour along the roads covered by the survey. Making certain assumptions about the cost of delay and taking the amount of traffic to be as in 1952, it seems likely that if the projected works succeeded in eliminating all delay at the sites concerned, about four per cent of the capital expenditure would be saved each year. Were the amount of traffic to increase, the savings would be greater.

Some Observations on the Use of Cycle Tracks

RN/2423/JEE

Observations have been made to determine what proportion of cyclists use cycle tracks. Tracks of different width were selected, some with good and some with poor riding surfaces, and observations were made on weekdays and on a Whit-Sunday.

It was found that tracks in good condition were used by about 90 per cent of cyclists on weekdays, whereas on the Whit-Sunday this figure dropped to about 75 per cent. Two of the observed tracks were in poor condition and only about 60 per cent of cyclists used them.

Counts were also made of the flow of motor traffic, but no correlation could be established between this flow and cycle-track usage.

Accident Reports as a Guide to Slippery Lengths of Road

RN/2457/BES

The note describes a simple method of detecting the more slippery lengths of road in the country, where surface treatment to improve skidding resistance could reduce accidents. Essentially, it involves studying accident reports with particular reference to the locations of accidents involving skidding on wet roads. Where clusters of skids occur the frequency of skidding is compared with some chosen standard of performance. A suitable standard would be the average rate of skidding in accidents in Great Britain, that is, the percentage of all wet-road accidents in which skidding is reported. At each site studied the number of accidents will generally be small, so that the significance of the frequency with which skidding occurs must be assessed statistically in order to make allowance for the possible effects of chance variations in the numbers of skids reported.

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These statistical tests are greatly simplified with the aid of a nomogram, which has been prepared to enable comparisons of frequencies to be made without the necessity for any calculations.

The use of the nomogram is illustrated by an example, and the theory underlying its construction is given in an appendix.

The method described has so far been tested in two counties: twelve sites thought to need treatment have been revealed. Ten of these sites were suitably treated, and the numbers of accidents fell; the other two sites awaited treatment for several months, and during this period the police reported a marked increase in the incidence of skidding.

It is suggested that similar accident studies could usefully be made in other areas. Although such studies may not reveal all road surfaces which tend to be slippery when wet, they will at least detect those roads which, by virtue of their accident records, are most in need of attention.

Halt Signs: The Effect on Driver Behaviour of Painting "Halt" on the Carriageway

RN/2545/JKS

At five sites in Buckinghamshire, existing halt signs were supplemented by the word "Halt" painted on the carriageway. Letters 2 ft high were painted in July 1954, and after these had worn away they were replaced in April 1955, by letters 4 ft high.

Observations were made of the numbers of vehicles approaching the junctions from the minor road, with and without vehicles passing on the major road, and counts were made of the flows on the major roads.

With the halt sign alone, just over 60 per cent of the vehicles on the minor road stopped before entering the major road. There was no change following the painting of the smaller letters, but a further 17 per cent of vehicles on the minor road stopped, following the painting of the larger letters. This change was statistically significant.

Preliminary Observations on the Problem of Mud Splashed on to Reflectors Marking the Edge of the Carriageway

RN/2613/AWC

The effectiveness of reflectors set in the kerb or on posts at the edge of the road may be severely reduced by mud splashed up by vehicles. Some preliminary investigations were made into (*a*) the possibility of screening reflectors on posts and (*b*) the type of cleaning required to remove dry mud from reflectors mounted in kerbs.

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At the test site a considerable amount of mud was projected in a direction making an angle of only about 22° with the forward direction and some making smaller angles. If this is generally the case it may be possible to screen reflectors on posts from most, but not all, of the mud, without cutting off the reflectors from the view of the approaching drivers at too great a distance. The mud splashed on to reflecting kerbs contained sufficient oil or similar material to prevent the kerbs being satisfactorily cleaned either naturally by rain or artificially by hosing down with water. The dirt was easily removed after lightly brushing with detergent and flushing with a hose.

ABSTRACTS AND REVIEWS

Analysis and Interpretation of Service Records.
A. T. WILFORD (London Transport).
J. Inst. Mech. Eng. (*Presented March* 8, 1955.)

The paper is concerned with records arising in the running and maintenance of a fleet of road passenger vehicles. Such data are inherently variable and unless this is appreciated much effort may be expended in seeking explanations of trends which are of a transient nature and of no real significance. It is necessary to discover the causes of the variability. In doing so it is often desirable to supplement the routine records by information derived from controlled tests; examples of such tests and their interpretation are given.

Records relating to failures and delays to vehicles in service are discussed and a system by which the level of failures is kept under close control is described. Fuel consumption records are then dealt with and the various factors which influence the figures are listed and dilated upon; particular mention is made of the effects of variations in temperature, loadings, traffic density, fuel and lubricating oil characteristics and "between-vehicle" variations. Service tests other than those relating to fuel consumption also receive mention and a method for assessing engine life, employing the "Life Table" technique as used in actuarial work is explained.

Organized Sales Activity.
J. J. KNAP.
Tydschrift voor Efficiente Bedryfsorganisatie, 1955; **10**, 178–187. (*In Dutch.*)

The author compares what he calls the classical, haphazard method of sales activity with a more rational and efficient system.

With the classical system the chances are that a relatively small group of regular customers is frequently visited, whereas a relatively large group of potential or irregular customers does not receive sufficient attention. Obviously with this type of sales activity the ratio of the cost to the results, the latter measured in terms of increased sales to new customers, tends to be high.

A more rational approach is to divide the district covered by one sales representative into a number of sub-districts in such a manner that each sub-district receives the same number of visits from the representative per year regardless of the frequency with which each individual potential or actual customer is to be visited. The author derives a simple formula giving the desired number of sub-districts and enlarges on various aspects of this system.

Universities in Management Planning and Control.

J. M. JURAN.

The Management Review, November, 1954; **43** (11), 748–761.

The authors discuss a number of principles which appear to be universally applicable to any problem in Management Planning and Control irrespective of the particular product, process, or function, involved. For example, the statement that "in any series of elements to be controlled, a selected small fraction, in terms of numbers, always accounts for a larger fraction, in terms of effect", leads on to a discussion of the importance of distinguishing vital factors.

Other principles put forward are concerned with the association between a few sweeping changes in the plan of operation and numerous small changes in administration; the freedom of choice of management; the function of forecasting; the fact that bad standards drive out good.

Institute of Management Sciences

The Institute of Management Sciences held its second National Meeting during October. A correspondent writes:—

The two-day session was devoted almost entirely to problems of management in business and industry. The programme committee made every possible attempt to avoid concentration on only a few problems areas of the industrial manager in the U.S.A. At the present time the most popular topics for management research are production planning, the use of computers and other data processing devices, and transportation. On production planning, there was a discussion of priorities in job-shop scheduling, the programming of heating oil, the use of forecasting in production planning. The

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discussions of computers were primarily concerned with evaluations of computers as an aid to management. The transport problems of both airlines and railroads were considered, as well as some basic results in the theory of transportation. The Institute of Management Sciences is vitally interested in integrating efforts in the psychological and social sciences with other types of management research: operations research, economics, industrial engineering, etc. This interest was reflected in a number of papers on organization theory, and an entire session devoted to the behavioral sciences.

CORRESPONDENCE

Sir,

MANAGEMENT SCIENCE

In connection with B.H.P.R's review of the first volume of "Management Science" (*Operat. Res. Quart.*, 7, (1) 23) I would suggest that it may be unnecessary to strain to find more than a nominal difference between management science and operational research. The difference between their progenitors, the Institute of Management Science, New York, and the Operations Research Society of America, may be like the difference between the Royal Statistical Society and the Association of Incorporated Statisticians. The same discipline, but with a different set of officers and orientation.

B.H.P.R. expressed the view that if there is a science of management as such, it will be one which must have as component techniques work study, job evaluation, time and motion study, and, in a rather broader content, operational research. This is a piece of tautology that emphasizes the urgent need for the British Standards Institution Committee covering *Definitions, Terminology and Symbols in Work Study*, to publish a report.

Work study has been defined as the analytical study of all forms of productive work in every type of industrial activity using, where appropriate, the techniques of method study (which includes motion study) and measurement (loosely called time study), with the object of improving productivity and working conditions.

Method study has been defined as the systematic analysis of existing or proposed methods of production as a basis for improvement. The object of method study is the improvement of working methods including (*a*) the flow of work, involving analysis of equipment plant layout and material handling procedures; (*b*) the movement of workers about the factory, involving analysis of work organization, the working environment, and the individual work place; (*c*) the planning of production, involving analysis of stores

procedure, machine loading and labour allocation and of the flow of paperwork.

Motion study has been defined as the detailed analysis of the motion of individual workers during particular operations, with the object of eliminating unnecessary motions and fatigue, and of defining the best working rhythm for the operation at any stage.

Work measurement has been defined as the recognized techniques used in the determination of the proper time to be allowed for the effective performance of a specified task. (The techniques commonly used include time study, synthesis form standard data and basic movement time data, analytical estimation and activity ratio studies. The work unit is used in place of time as a measure of work content.)

Job evaluation (where a job is defined as a recognized collection of duties assigned to an individual) involves: (*a*) a thorough examination of the job to be assessed; (*b*) the preparation of a job description to record its characteristics in a manner most suited to the method of assessment (the preparation of a job analysis may also be involved, setting out the requirements of the job under various factor headings); (*c*) the comparison of one job with another by methods varying from overall judgment of the whole job to a detailed consideration of a number of factors and sub-factors; (*d*) arranging jobs in a progression according to some numerical measure or by broad comparison; (*e*) relating the progression of jobs to a money scale to obtain their relative worth.

The four main groups of job evaluation systems in use in industry, ranking, classification, points rating and factor comparison, may be found enumerated in Appendix IV of the *Report of the Royal Commission on the Civil Service* 1953–55.

I take it that the omission of reference to organization and methods is due to its being considered as operational research applied to paper pushing.

Finally, with regard to the comments following the observation that the two American societies have a significant proportion of their members drawn from the universities and research institutes, a glance at the current membership list of the Operational Research Society shows that 22 per cent are clearly in that category. Six sample pages of the 1955 Directory of the *Operations Research Society of America* give a value of 26 per cent.

Yours truly,

R. S. GANDER.

Institute for Engineering Production,
Birmingham, 15.

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OPERATIONAL RESEARCH QUARTERLY

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OPERATIONAL RESEARCH SOCIETY

SESSION 1956/57

THE FOLLOWING MEETINGS have been arranged for the coming session. All meetings will start at 5 p.m. (tea 4.30 p.m.) in the Council Room of the Agricultural Research Council, 15 Regent Street, London, S.W.1.

24 October, 1956. Annual General Meeting. This will be followed by an ordinary meeting. Speaker: Professor A. Tustin (Imperial College, London), *The Behaviour of Systems of Interdependent Quantities with Special Reference to Economic Stability*. Chairman: Sir William Slater.

12 November, 1956. Speaker: Dr. D. A. Boyd (Rothamsted Experimental Station), *A Scrutiny of the British Potato Crop*. Chairman: H. G. Jones.

10 December, 1956. Speaker: Dr. A. Robertson (Institute of Animal Genetics, University of Edinburgh), *Operational Research in Animal Breeding*. Chairman: R. T. Eddison.

16 January, 1957. Speaker: N. T. J. Bailey (Oxford University), *Operational Research in Hospital Planning and Design*. Chairman: Sir Owen Wansbrough-Jones.

21 February, 1957. Speaker: R. T. Burns (Social Science Research Centre, Edinburgh University), *A Study of Top Management Groups*. Chairman: Dr. T. E. Easterfield.

March, 1957. Open meeting. Date and subject to be announced.

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Professor P. M. S. Blackett, F.R.S.

PROFESSOR BLACKETT is to be congratulated on his election as next year's President of the British Association for the Advancement of Science, at the Association's meeting in Sheffield at the beginning of September. Professor Blackett's war-time work on operational research was seminal, and he was one of the original members of the Operational Research Club in 1950. His paper "Operational Research" was in fact the first article to be printed in this *Quarterly*.

O. R. Q. SUBSCRIPTION RATES

It is with regret that the editors are compelled to announce an increase in the rates of subscription to the *Operational Research Quarterly* to take effect from the end of the current volume. From the first issue of 1957 onwards, the new rates will be:

	British Commonwealth	*Other Countries*
Surface mail	20*s.*	30*s.* ($4)
Air mail..	40*s.*	60*s.* ($8)
Single copies (*surface mail*)	6*s.*	8*s.* ($1·20)

There has been no previous increase in the subscription rate in British Commonwealth countries since our foundation in 1950, and only one increase in non-Commonwealth countries, to absorb higher postal charges. During these seven years we have grown in size from 72 pages in 1950 to 176 in 1955, an increase of 144 per cent. But higher costs have been counterbalanced by a steadily growing circulation, which now stands at over 1,000 paid subscriptions.

The latest increases in printing costs and postal rates, however, leave us no choice but to increase our rates as noted above. At the same time we have revised our arrangements for supplying back issues to ensure that even those (the majority) which are now out-of-print will be available to purchasers wishing to complete their files. Such issues are now available in photographic reproduction from a London firm, to whom we shall be pleased to refer enquirers.

The change in subscription rates makes no difference to the arrangement whereby members of the Operational Research Society receive copies as part of the service to subscriptions to the Society.

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RISK AND HAZARD

Influence of Training on the Performance of Bus Drivers

by

JOHN COHEN,* E. J. DEARNALEY* and C. E. M. HANSEL*

1. *Introduction.* 2. *Aim of Enquiry.* 3. *Experimental Procedure.* 4. *Results: (i) Estimates and Performance of Less and More Experienced Drivers; (ii) Maximum Risk-taking Levels; (iii) Margins of Safety and Hazard.* 5. *Results, Conclusions and Discussion.*

Introduction

ON THE face of it, the problem to be studied in this paper might seem to be of a rather specific character. It arose, however, in the consideration of a wide variety of questions that fall within the range of what we have called subjective probability and risk-taking. It seems important to recognize that when, for example, a man is driving a car or when a pedestrian is crossing a road his behaviour has something in common with behaviour in a diversity of other conditions which are characterized by an element of uncertainty. We have elsewhere[1, 2, 3, 4] tried to show that all forms of judgement or decision made when the information available is incomplete, just as all kinds of performance undertaken by a person when he is not absolutely certain that he will succeed, fall within the scope of the study of subjective probability and risk-taking. Our previous studies embrace changes, during the period of mental development, in judgements made in states of uncertainty. These states of mind arise, for example, in our understanding of the way distributions are built up, the nature of statistical independence and the various processes involved in the knowledge gained from a sample and in the interpretation of words with a quantifiable meaning. We have also investigated confidence in decisions at different ages, the subjective element in gambling, and various forms of risk-taking with and without danger.[5]

Aim of Enquiry

Consider a situation which arises frequently in the life of a motorist. There is an opening in the traffic in front of him. He must decide whether this gap is large enough for him to drive his car through or not. The simplest case would arise when other vehicles in the traffic are stationary, perhaps in a queue at traffic signals. Suppose his car is 5 ft wide; would he drive through an opening of 5 ft wide plus

* Department of Psychology, University of Manchester.

half an inch, thus allowing a clearance of only half an inch? If he would not attempt this task, what is the smallest size of a gap he would attempt to drive through? In other words, what is the smallest clearance he would allow beyond the width of the car? We do not imply that motorists in general know the exact size of their vehicles. Indeed we suspect that such precise knowledge is unusual. Nevertheless they have to decide when not to drive through a gap.

The task of driving through a gap in the traffic may present varying degrees of difficulty to any motorist who is going to attempt it. We can define "degree of difficulty" in terms of the proportion of times he thinks he will succeed at any size of gap or clearance. The extreme degree of difficulty would be one at which he thinks it would be quite impossible to drive through, for instance, if he believes the gap is less than the width of the car. There is thus always some subjective probability of success or failure which represents the ease or difficulty of the task.

We use the word "risk" to denote any attempt at performance when the person is not sure he will succeed or when his subjective probability is less than 1·0. If we have a measure of a person's subjective probability of success or failure we can use it as a measure of risk-taking. An attempt which constitutes a risk for one person may not signify a risk for another person whose subjective probability for the attempt is 1·0.

The idea of risk is thus based on an individual's feeling of his likely success or failure. The concept of "hazard", on the other hand, we use to refer not to what the individual *thinks* about the situation, but to his *actual* success or failure in performance. There are, of course, degrees of hazard just as there are degrees of risk. In our example, a motorist who thought he could always drive through a gap of 5 ft plus half an inch successfully would not be taking a risk if he attempted to drive through. But if we tested his driving skill and found that he failed in a proportion of his attempts, the situation would hold some hazard for him. The proportion of his failures in actual performance provides a measure of hazard in these circumstances.

In the study[4] of pedestrian behaviour we attempted to measure levels of risk-taking in everyday situations when failure might be rather serious. In the present paper our chief aim is to examine differences in these risk-taking levels attributable to training in the task.

Experimental Procedure

Our experiment is based on two groups of trainee bus drivers. One group consisted of beginners in their first week of instruction, and

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the other, though still under training, had all passed the Ministry of Transport Public Service Vehicles Licence test in the week before the experiment. The average duration of the training period was six weeks.

The experiment was carried out at the Manchester Corporation Transport Department's Training School for Bus Drivers. The task was to drive an 8-ton double decker bus between two wooden posts 6 ft high. These posts were 3 in. square in section and painted with black and white bands. The difficulty of the task could be varied by altering the distance between the posts. The task was included in the training course carried out in the presence of an instructor.

Two lines, 12 ft long, were marked on the ground in the training school yard. One line was a starting line. The other, parallel to it and 12 ft away, was marked in 6 in. intervals, the posts being placed on this line. These marks were such that they could not be seen from the driver's seat. The experiment began with an 8 ft wide bus parked with its front to the starting line facing the centre of the gap.

We first estimated the driver's subjective probability of success at differing degrees of difficulty. The driver was instructed to sit in his seat. He was told that he was to take part in a test which consisted of driving between the two posts but that first we wished to find out how well he thought he could do it.

The experimenter pointed out that the gap between the posts was too narrow for the bus to pass through but that they would be moved apart a little at a time. At each distance the driver was asked to say how many times out of five he thought he could get through without touching the posts. An instructor moved the posts apart 6 in. at a time. This was reduced to 3 in. for the more experienced group. The posts were moved alternately so that the bus was always facing the centre of the gap. At each distance the experimenter asked the driver how many times out of five he thought he could get through. This was continued until the driver said he could succeed five times out of five.

The next step was to determine the driver's maximum risk-taking level. The posts were replaced seven feet apart. It was explained that they would be moved apart as before and that as soon as he was sure he could manage it he was to drive between them and circle round back to the starting point. The driver was told that now he must actually drive between the posts but that on no account must he touch them. The experimenter and an instructor moved the posts apart as before until the driver signalled his intention to make an attempt. The distance apart was noted and the

driver drove through and back to the starting point. The driver was allowed as much time as he wished.

Finally we measured the driver's actual performance at different distances between the posts. Because of limitation of time this could only be done for three distances. The experimenter chose distances on the basis of what he had seen of the driver's performance in the previous test. The three distances chosen were such as to give a low, a high and an intermediate level of success. If in the case of any driver these were not realized he made another set of attempts. The posts were placed at the first distance and the driver told to make five attempts at that distance. This was repeated for the two other distances. If he wished, the driver was allowed a short rest between each set of five attempts. For each attempt the experimenter and instructor each stood by one of the posts in order to move them quickly aside if they were hit so as to avoid excessive damage. The time taken for the experiment varied from 15 minutes for the more experienced drivers to an hour for some of the beginners, the average time being half an hour. The whole programme was completed during June and July, 1955.

The length of time required by some of the beginners was due to their lack of proficiency in clutch control and consequent repeated stalling of the engine. This held up the experiment seriously because to put the full lock on after passing between the posts, it was necessary to use half clutch. Some of the beginners were also not very proficient in steering. An instructor provided assistance in bringing the bus back to the starting line when necessary but did not help during an attempt. The experiment had to be abandoned in the case of three beginners because of their lack of proficiency.

Three sets of observations were taken for each subject at distances between the posts varying from 7 to 12 ft. The distances were in intervals of 6 in. for the beginners and 3 in. for the more advanced trainees, the intervals increasing from smaller to larger distances.

(i) At each particular distance between the posts, the driver was asked to say how many times he thought he could get through without touching them.

(ii) (*a*) The smallest distance between the posts which the driver attempted to drive through: (*b*) His success or failure in this attempt.

(iii) The proportion of successes in driving through the posts as these were placed at varying distances apart.

Observations were made of the intervals as they increased in size from smaller to larger distances in the case of (i) and (ii) but

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not always in the case of (iii). The observations were made in the order (i), (ii), (iii), so that any experience gained in (iii) would not affect (i) and (ii).

A word of elucidation is needed about the scale. We have used a scale of 0 to 5 and the subject is limited to a choice of one of six values on this scale. Several questions, however, must be faced. First, is it natural for people to express their judgements in terms of the type of scale we have used? Or is it the case that people generally prefer to say that they think they can either do or not do the task? We did not encounter any difficulties on the drivers in expressing their judgements on this scale. In previous studies with children and adults we have hardly ever found anyone who felt that such a scale made his judgement artificial. We do not therefore feel that an artefact is being imposed on the drivers. The consistency of their estimates bears this out.

Second, is it possible that significantly different results might have been obtained had we employed scales ranging from 0 to 10, 0 to 100, or 0 to 1000? Possibly a subject who says he could succeed five times out of five might say, on a different scale, that he would succeed 95 or 99 times out of a hundred. In so far as the subject has any doubts at all about his capacity, this might be manifested on a scale with a large enough range. On the other hand, a subject in spite of any lurking misgivings might still say he could succeed 100 times out of 100 or 1000 out of 1000. The question is whether there are upper and lower thresholds on the scale of subjective probability. Above the upper threshold the subject would be completely certain of success and below the lower threshold he would be equally certain of failure; in the intermediate zone he would be uncertain. There might also be differential thresholds.

Third, there is the question whether a linear mathematical transformation of one scale into another is psychologically justified. A subject who says four out of five, when asked to estimate on a scale of 0 to 100 might say 80 out of 100 or any value between 70 and 90 or he might say 99 out of 100. In the last case, this would be his best means of conveying an element of slight uncertainty. These questions of the equivalence of psychological and mathematical scales can only be answered by experimental study, which we are now undertaking.

Results

(i) *Estimates and Performance of Less and More Experienced Drivers.* In Tables I and II we show, for the two groups of drivers respectively, the average number of times they think they would

succeed out of five attempts, their actual performance in five attempts, and the number who attempt to drive through at each distance between the posts.

We see from columns *ii* and *iii* that both the estimates and the actual performance of the experienced drivers are greater than those of the inexperienced. From column *iv* we can see the relationship between estimates and the performance. Both groups tend to underestimate their capacities when the task is relatively easy and to overestimate their capacities when the task is relatively hard.

Columns *ii* and *iii* of Tables I and II respectively are plotted in Figures 1 and 2.

TABLE I

MEAN ESTIMATE OF SUCCESS AND ACTUAL PERFORMANCE OF INEXPERIENCED DRIVERS (n = 15)

WIDTH OF BUS 8 FEET

Distance between Posts		Estimated Successes out of 5	Successful Performances out of 5	Error of Estimate	Number who would Attempt Task
Feet	ins.				
i		*ii*	*iii*	*iv*	*v*
7	0	0·0	0·0	0·0	1
7	6	0·0	0·0	0·0	2
8	0	1·0	0·0	+1·0	4
8	6	2·0	1·0	+1·0	8
9	0	3·0	2·2	+0·8	10
9	6	4·2	3·5	+0·7	14
10	0	4·5	4·8	−0·3	15
10	6	4·6	5·0	−0·4	15
11	0	4·7	5·0	−0·3	15
11	6	4·9	5·0	−0·1	15
12	0	5·0	5·0	0·0	15

TABLE II

MEAN ESTIMATES OF SUCCESS AND ACTUAL PERFORMANCE OF MORE EXPERIENCED DRIVERS (n = 15)

WIDTH OF BUS 8 FEET

Distance between Posts		Estimated Successes out of 5	Successful Performances out of 5	Error of Estimate	Number who would Attempt Task
Feet	ins.				
i		*ii*	*iii*	*iv*	*v*
7	0	0·0	0·0	0·0	0
7	3	0·0	0·0	0·0	0
7	6	0·3	0·0	+0·3	0
7	9	1·2	0·0	+1·2	1
8	0	2·0	0·0	+2·0	3
8	3	2·5	1·1	+1·4	7
8	6	3·4	3·0	+0·4	10
8	9	3·7	4·4	−0·7	13
9	0	4·3	4·9	−0·6	14
9	3	4·4	5·0	−0·6	14
9	6	4·5	5·0	−0·5	15
9	9	4·7	5·0	−0·3	15
10	0	5·0	5·0	0·0	15

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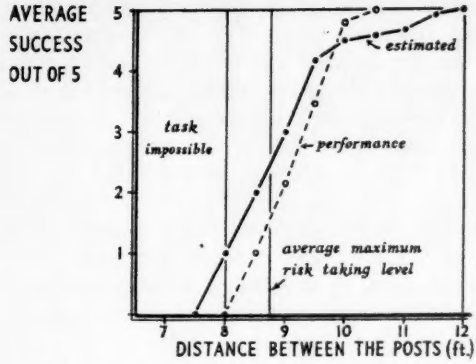

FIGURE 1. *Mean estimate of success and actual performance of inexperienced drivers* ($n = 15$).

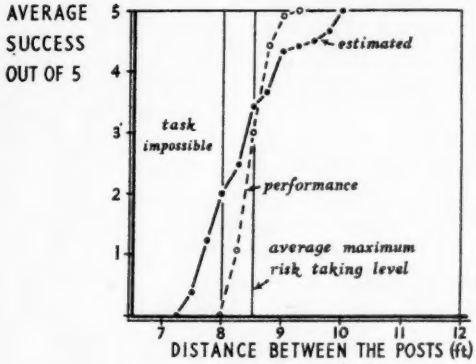

FIGURE 2. *Mean estimates of success and actual performance of more experienced drivers* ($n = 15$).

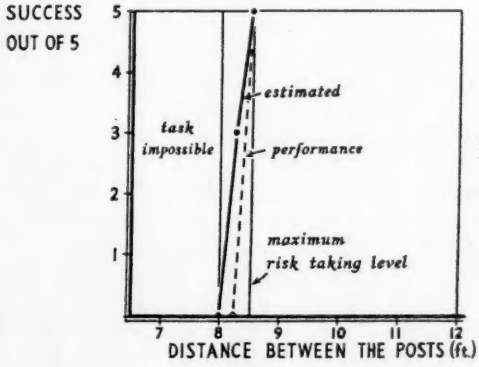

FIGURE 3. *An experienced driving instructor.*

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It is evident from Figures 1 and 2 that the relationship between estimates and performance remains much the same for both groups of drivers. The essential difference between the two groups consists in the fact that the more experienced drivers attain full success at smaller distances, as indeed we should expect. The effect of the training shows itself in superior performance rather than in more realistic judgements of their own skill. This is brought out in column *iv* of Tables I and II. The error of estimate of the trained drivers is, if anything, larger than that of the untrained. We should of course bear in mind that at this stage, the "experienced" drivers have not completed their full training and that they are not yet regarded as qualified drivers by the Manchester Corporation, although the Ministry of Transport regards them as fully qualified.

In both groups we find that at the smallest gap when they are certain they will succeed, i.e. they do not feel they are taking any risk, they do in fact invariably succeed with a still smaller gap. The relevant pairs of figures are 10 ft and 8 ft 6 in. for the experienced and 12 ft and 10 ft 6 in. for the inexperienced. These are average values. There are, of course, variations from driver to driver. In fact eight out of the inexperienced and five of the experienced drivers thought they would always succeed at a distance smaller than the one at which they did, in fact, always perform successfully.

From the point of view of safe driving the ideal place for the intersection between the two curves should be on the line "b". That would mean that at the first magnitude of the task he would be prepared to undertake, his judgements would be realistic and he would be fully aware of his limitations. Therefore at any level of difficulty that he would attempt, he would be cautious and underestimate his capacities. We find that the point of intersection of the two curves is closer to line "b", i.e. the maximum level of risk taking, in the case of the trained as compared with the untrained drivers. Ideally too, for maximum safety in driving, line "b" should coincide with a performance level of 100 per cent. In fact line "b" is closer to this level in the case of the experienced drivers. The conditions of the experiment did not involve the common hazards of the road and we should therefore not expect maximum risk-taking levels to coincide with a 100 per cent level of performance. The estimates and performance of the experienced driving instructor that we tested exemplifies these conditions. (See Figure 3.)

(*ii*) *Maximum risk-taking levels.* In our previous study of pedestrian behaviour we used the expression "maximum risk-taking level" in the sense of a value of subjective probability of risk which a person will rarely, if ever, take. In the circumstances of that enquiry we

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could simply observe the time that a person allowed himself to cross the road before the arrival of an oncoming vehicle. We could not, in those circumstances, obtain a measure of his subjective probability of successful performance by asking him in advance whether he thought he was likely to arrive intact at the other side of the road. In the present study we have a direct measure of maximum risk-taking level in terms of the smallest gap between the posts which a driver will attempt. The values for the individual drivers, inexperienced and experienced are shown in Tables III and IV. The average maximum risk-taking level for the inexperienced drivers is 8 ft 9 in. At this value they estimate in advance that they could succeed in 54 per cent of the attempts; in actual fact they succeed in 40 per cent of the attempts. The corresponding figures for the experienced drivers are 8 ft 6 in., 60 per cent and 50 per cent. Although the differences between the values for the two groups are not statistically significant they do indicate that the more experienced drivers take less risk although their clearance is smaller than that of the inexperienced. This is shown by the fact that their success in performance is 50 per cent as compared with the inexperienced drivers. We should add that the variation in maximum risk-taking level is significantly greater for the inexperienced than for the experienced drivers.

Three of the inexperienced and six of the experienced drivers took no risk. Of these drivers who took no risk only one in each

TABLE III

SMALLEST GAP BETWEEN POSTS WHICH INEXPERIENCED DRIVER WOULD ATTEMPT, HIS ESTIMATED SUCCESS AND ACTUAL PERFORMANCE

Driver	Smallest Gap which Driver would Attempt		Estimated Percentage Success	Performance Percentage Success
	ft	in.		
1*	9	0	100	80
2*	8	0	40	0
3	10	0	100	100
4*	7	0	0	0
5*	8	0	0	0
6*	8	0	100	0
7	9	6	40	100
8	9	6	40	100
9*	8	6	60	100
10	9	6	80	20
11*	8	6	60	0
12*	8	6	40	0
13*	9	0	60	0
14*	8	6	40	20
15	9	6	40	80
Means	8	9	54	40

* These drivers failed at their maximum risk taking level.

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TABLE IV

SMALLEST GAP BETWEEN POSTS WHICH EXPERIENCED DRIVER WOULD ATTEMPT, HIS ESTIMATED SUCCESS AND ACTUAL PERFORMANCE

Driver	Smallest Gap which Driver would attempt		Estimated Percentage Success	Performance Percentage Success
	ft	in.		
1*	8	3	100	40
2	8	9	100	100
3*	8	3	100	0
4*	8	6	100	20
5*	8	6	0	0
6*	8	3	40	20
7*	8	0	100	0
8*	8	0	100	0
9*	8	3	60	60
10	9	6	0	100
11*	8	9	40	100
12	8	6	80	100
13*	7	9	0	0
14	8	6	80	100
15	8	9	0	100
Means	8	6	60	50

* These drivers failed at their maximum risk-taking level.

group was not involved in any hazard, that is, he was always successful at his maximum risk-taking level. We must regard him as the best driver in his group from the point of view of road safety.

Four of the inexperienced and six of the experienced were not involved in any hazard. Only one in each group took no risk, that is, would not undertake the task at his maximum risk-taking level unless he thought he would always be successful.

Nine of the inexperienced and four of the experienced drivers both took a risk and were involved in hazard.

(*iii*) *Margins of Safety and Hazard.* The maximum risk-taking level is a measure of a person's subjective probability of success at the magnitude of a task which he will first attempt. He tells us, in other words, the percentage of times he thinks he will succeed at the magnitude of the task he will first undertake. This measure is independent of the nature of the task and of the physical units employed, so that different tasks may be compared in respect of an individual's risk-taking levels. Different individuals may also be compared with respect to the same or different tasks.

The determination of a person's maximum risk-taking level does not give us complete information about his behaviour in a given task and it therefore needs to be supplemented by an additional measure. For example, two drivers may first declare that they will always succeed when the gap is increased to 9 ft. One of

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them is prepared to drive through a gap of this magnitude, but the other will only undertake a gap of 9 ft 3 in. They have the same maximum risk-taking level, but their conduct is different. Similarly, two drivers may both say that they will never succeed at a gap of 8 ft 6 in. This is the smallest gap which one of them will attempt to drive through, whereas the other will attempt a gap of 8 ft 3 in.

At the two extremes of the scale, two persons may thus have identical levels of maximum risk-taking but they will not necessarily act in the same circumstances.

In calculating the safety margin of a performance we subtract the smallest distance at which the driver is sure he can succeed five times out of five from the magnitude of the task in the distance in any given performance. This measure will be in units of feet and inches but would have the advantage of being continuous. There would be only one safety margin for every performance and only one performance would correspond to any safety margin. The measure would be positive when no risk is being taken, that is, when the person thinks he could succeed at all five attempts, and negative when some risk is involved, that is when he thinks he may not succeed in all five attempts.

We can apply a similar measure to hazard. This may be calculated in the same way as safety margin except that we subtract the smallest distance at which a person could actually succeed every time out of five from the distance in the performance in question.

TABLE V

CLEARANCE, SAFETY MARGINS AND HAZARD MARGINS AT THE MAXIMUM RISK-TAKING LEVELS FOR INEXPERIENCED DRIVERS

Driver	Clearance	Margin of Safety*	Margin of Hazard*
	ft	ft	ft
1	1·0	0·0	−0·5
2	0·0	−1·0	−1·5
3	2·0	0·5	0·0
4	−1·0	−1·0	−2·0
5	0·0	−1·0	−2·0
6	0·0	0·0	−2·5
7	1·5	−2·5	0·0
8	1·5	−2·5	0·0
9	0·5	−1·0	0·0
10	1·5	−0·5	−1·0
11	0·5	−1·5	−1·5
12	0·5	−1·0	−1·0
13	1·0	−0·5	−1·5
14	0·5	−1·0	−1·0
15	1·5	−1·5	−0·5
Means	0·73	−0·97	−1·00

* The greater the negative value, the larger the risk or hazard.

TABLE VI

CLEARANCE, SAFETY MARGINS AND HAZARD MARGINS AT THE MAXIMUM RISK-TAKING LEVELS FOR MORE EXPERIENCED DRIVERS

Driver	Clearance	Margin of Safety*	Margin of Hazard*
	ft	ft	ft
1	0·25	0·75	−0·50
2	0·75	0·25	0·00
3	0·25	0·50	−0·75
4	0·50	0·00	−0·75
5	0·50	−0·50	−0·50
6	0·25	−1·00	−0·25
7	0·00	0·25	−0·50
8	0·00	0·00	−1·00
9	0·25	−0·25	−0·25
10	1·50	−0·50	0·50
11	0·75	−1·25	0·00
12	0·50	−0·25	0·00
13	−0·25	−0·50	−0·50
14	0·50	−0·25	0·00
15	0·75	−1·00	0·00
Means	0·43	−0·25	−0·30

* The greater the negative value, the larger the risk or hazard.

It is the difference between what he *could* demonstrably do and what he actually does.

Margins of safety and hazard calculated in this way are shown at the maximum risk-taking level, in Tables V and VI. The means of the two groups of drivers differ significantly, although no significant difference appears between the means for clearance. The value of t for the difference in safety margins is 2·82 (d.f. = 28), $p<0{\cdot}01$; and for the difference in hazard margin t is 2·96 (d.f. = 28), $p<0{\cdot}01$.

Results, Conclusions and Discussion

The essential feature in this type of investigation is that it enables us to take account of the state of mind of a driver and of the significance of his actions for him. The alternative would merely be to record observations of drivers' behaviour which may include psychologically heterogeneous elements. Individual drivers must be studied separately otherwise the specific factors, such as awareness of risk, which influence them will either not appear at all or be obscured in the averages derived from a group. It is the elucidation of this kind of psychological factor which will help us to understand sudden or dramatic changes in a driver's behaviour as may happen if he gets into a state of panic. It is not, for example, the objective speed of vehicles, by itself, which is the crucial element but the speed at which the driver feels uneasy and at which his performance begins

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to deteriorate within a given constellation of circumstances. This speed varies from driver to driver.

Our main specific conclusions are as follows:

(i) On the average, the more experienced drivers had higher minimum margins of safety and hazard. That is, they took less risk and were involved in less hazard. As a group they varied less than the inexperienced drivers.

(ii) The two groups did not differ with respect to the size of clearance, and the proportion failing in performance at their maximum risk-taking levels respectively was the same in both groups.

(iii) On the average both groups would attempt a task when they were far from certain they would succeed.

(iv) Both groups tended to underestimate their capacities when they thought the task was relatively easy and to overestimate their capacities when they thought the task was relatively hard.

(v) The differences between the two groups was not large but we must bear in mind that what we have called the "experienced" group had only had about six weeks' more training than the inexperienced group. The trends in the differences are all such as we should expect to be the outcome of training. These trends are:

(*a*) a tendency to take less risk,

(*b*) a tendency to be involved in less hazard,

(*c*) a tendency for variation in the group to diminish,

(*d*) a tendency for estimates of performance to become more realistic at the maximum risk-taking level and also for the maximum risk-taking level to approach the point at which they think they will always succeed and will, in fact, always succeed.

These trends are borne out by the estimates and performance of the driver-instructor.

(vi) The superiority of the experienced group shows itself in improvement of skill in steering and manipulating the bus rather than in better judgement of their driving capacities. In fact a proportion of both groups attempted the physically impossible task of driving the bus through a gap narrower than its width. The experienced driver-instructor, however, showed his superiority chiefly in being able to judge accurately when it was and was not possible to drive through a given gap. Thus when driving a bus 8 ft wide he could not succeed at a gap of 8 ft 3 in. and did not attempt it whereas he could always succeed at 8 ft 6 in. and did attempt it. It would seem advisable to include training in making judgements of this character in a wide variety of conditions during the period of training.

Finally we should like to draw attention to two implications of this investigation, one for a psychological approach and the other for the general problem of road safety. The special contribution which psychological study can make is one which takes an entirely different point of departure from that taken by the engineer or physiologist. In effect it means that the observations in the first place are made from the subject's own perspective. This is necessary because his behaviour is partly at any rate determined by the way he perceives and understands the situation. The perspective will of course vary from subject to subject. What seems easy or safe to one may seem hard or dangerous to another. The present study shows that a purely psychological approach can be combined with a proper experimental design and a suitable statistical evaluation of results.

Behaviour at zebra crossings may serve to illustrate further certain aspects of a psychological approach.

Pedestrians in Chingford complained that they slipped on zebra crossings. Investigators from the Road Research laboratory reported that "the stripes are no more slippery than the rest of the road" and concluded that the pedestrians slipped for psychological reasons: the stripes *looked* slippery. The situation at a zebra crossing when a pedestrian is contemplating crossing the road and a motorist is approaching is one the outcome of which depends on mutual guesses of each other's intentions rather than on physical factors or legal rights. Zebra crossings can only become safe to the extent that elements of subjective uncertainty are reduced.

As far as the incredibly complex problem of road safety is concerned, we take the view that an understanding of it can only come by means of patient experimental study. The present enquiry emphasizes, among other things, the importance of the learning process. Road behaviour differs from most other ordinary activities in that failure may mean disaster. Learning, therefore, takes place without full knowledge of results. Only rarely can we learn from our failures, for example, from failure in overtaking. It follows therefore that training to drive should include situations such as this which yield knowledge of results in safe circumstances. The emphasis in learning to drive a car is at present on learning to manipulate the vehicle. What is equally important is learning to judge the degree of hazard in a situation. This implies training in judging clearance, speed, distance and so on. We wonder how many motorists, having mastered their Highway Code, will follow its advice to turn into the direction of the skid when involved in one the first and perhaps the last time. The vast majority would be in a state of panic. The only way to avoid this is to provide enough training for the correct

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response to be made as the result of habit. How many motorists in the course of their driving never take a risk in the way we have defined it? Again, how many motorists are there who though they never take risks in our sense often get involved in grave hazards? Answers to these questions are unlikely to be found in the statistics of the Ministry of Transport, useful as these undoubtedly are for answering Parliamentary questions. *A priori* panaceas in terms of reduced speed limits, wider roads, cars in better state of repair, or holding aloft ideas of courtesy are not likely by themselves to solve these problems. Nor is the proposal that a Royal Commission on Road Safety be appointed to establish the facts a solution since the kind of facts that need to be determined require a long term research programme into the behaviour of road users and could hardly be established by a body such as a Royal Commission.*

We are inclined to suggest that one of the first steps to be taken in an experimental programme should be a frank enquiry carried out on the lines of the Kinsey Reports† on human behaviour in another sphere fraught both with risk and hazard. The confidential character of the enquiry would probably disclose in a startling fashion the enormous risks habitually taken by motorists on the road. By checking the judgements of a sample of them in a way similar to our study of bus drivers, the extent of hazard to which they expose themselves could also be estimated. An enquiry on these lines has now been initiated.

ACKNOWLEDGEMENTS

We are very grateful to the Manchester Corporation Transport Department and in particular to Mr. J. F. T. Webb, Personnel Officer, and his staff at the Bus School for the facilities and help they afforded us in carrying out this investigation.

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* In Ministry of Transport statistics for 1953 (Table 39) these factors are regarded by the police as contributing to the accident in almost 10 per cent of the cases.

† This should not be taken to imply that we endorse the methods or all the conclusions of these Reports.

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STATISTICS AND HOP GROWING EXPERIMENTS*

by

S. V. CUNLIFFE†

WHEN I was asked to speak to the Operational Research Society, various subjects were proposed relating to the brewing process. You may perhaps realise that Guinness' were one of the earliest firms to apply operational research techniques (although they were not then known by that name). At about the beginning of this century the firm adopted a method of recruitment for its brewers, which it has continued ever since. It likes to recruit honours men from either Oxford or Cambridge, and in addition to high academic qualifications, it likes its brewers to have excelled in some other field. These brewers spend several years learning about the brewing process and are then put in charge of various sections where they are given considerable scope for experimentation, and it is from this experimentation that Guinness' interest in statistics has grown. In 1899 Gosset came into the Brewery under this scheme, having obtained firsts in both mathematics and chemistry. He found in the course of his work that the mathematical theory that he had acquired was often inadequate to deal with the small number of experiments that could normally be carried out in a large scale plant. There were two things that could be done: one was to have pilot plants, and the other was to try and develop mathematical techniques for coping with small samples. Both these steps were adopted.

We now have experimental breweries on a laboratory scale at both Park Royal and Dublin. At Dublin we have a pilot brewery and an experimental maltings, and at Park Royal we have one sixth of our capacity that can be cut off from the whole for use as an experimental unit. In addition Gosset pursued the possibility of adapting mathematical techniques to experimentation and, as you

* Paper given to the Operational Research Society, London, 25 November, 1955.
† Arthur Guinness Son & Co. (Park Royal) Ltd.

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know, derived, amongst other things, the "t" test. Student's "t" test has remained one of the important statistical tools in experimentation. Gosset concerned himself with problems not only within the brewery, but also with many others directly and indirectly associated with the industry. He did a lot of work on the numerous agricultural experiments carried out by Dr. Beaven at Warminster when he was engaged on barley breeding, and assisted in the statistical work involved in propagating the Archer barleys that held the field for so long in this country.

The brewery in which I work is at Park Royal. It was built just before World War II, the first brewings taking place in 1937. Gosset was its first head brewer. The Statistical Department as such at Park Royal did not come into existence until after the war, when I joined the brewery in 1947. Since then we have built up a very small department which deals with a multitude of subjects. Dublin have their own Statistical Department which has been in existence ever since Gosset's day.

Today I am asked to concern myself with experiments on our hop farms. Guinness' own hop farms are in Sussex and Kent, at Bodiam and Teynham, and in addition we have a farm at Braces Leigh in Worcestershire. The Bodiam hop farms are not on ideal hop growing soil and therefore our yields there are not high.

All the farms are essentially meant to be run as commercial propositions, and particularly until recently, statistically designed experiments have not in general been carried out. The cultural operations are traditional, and little planned experimentation was done on cultivation, manurial treatments, methods of picking, drying etc. However, during the past five years or so, some experiments have been carried out that would perhaps interest members of the Operational Research Society. Before I start to describe them I should like to state that in all the experiments I am going to consider I come in as the statistician. The experiments are the combined effort of many people—the farmer, the chemist, the botanist, the physicist, the engineer, the plant pathologist etc. (this is, of course, the case with all operational research).

The statistical techniques involved in what I am going to talk about today are all perfectly straightforward—there is nothing new. In fact this is essentially a talk designed to try and show how, by applying orthodox statistical tests to particular problems, solutions may emerge more easily. It is written for the newcomer to operational research who is wondering whether this mystical method holds any treasures for him. You more advanced and erudite gentlemen who want to argue about theory may, I fear, be bored.

The first experiment that I want to discuss is concerned with machine picking.

Traditionally, hops were picked by hand and Guinness still have several thousand pickers who come down yearly (mainly from Stepney) to our gardens at Bodiam, and from Midland towns to Braces Leigh. Recently however, with full employment and paid holidays, the number of hop pickers available has dwindled, and we, together with most other large growers, have had to install machinery to pick the hops. We have designed our own machine and have also bought prototypes from various manufacturers, but I think we can say that we are not entirely satisfied with anything we have had so far—the machines are all too rough. The hop cone is very delicate and the machine method of tearing the cones off the bine (the plant growing up the strings) tends to damage the cone and knock out the vital resins. It is these resins that give the hop its brewing value—they are put into the beer for preservative purposes and for flavouring.

With machine picking, the bines in the garden are cut and fed into the machine on hooks. The machine then tears off the cones, also incidentally often tearing off a mass of leaf, stalk, etc., which we do not require, sorts the leaf etc. out, and then feeds the hops onto a belt from which they are conveyed into pokes (which are sorts of large sacks) for carrying to the oasts where the hops are dried and then pressed into pockets.

Some experiments have been carried out to investigate the effect of machine picking on subsequent yield. As already mentioned, in order to pick by machine we cut the bines off near the ground and carry them to the machine; this is in contrast to the hand picking method where the strings are cut down from the overhead wirework in the garden, the hops being picked on the spot and the bine left to die back naturally. We have now a considerable amount of evidence that cutting the bine results in a somewhat lower yield in the following year. This can be seen from Table I. In the second year, however, the plant appears partially to recover.

TABLE I

EFFECT OF MACHINE PICKING ON SUBSEQUENT YIELD

Garden	Method of Picking	Yield of Green Hops lb per Hill in 1952	Yield of Green Hops lb per Hill in 1953
Valley	Machine Picked in 1951	3·03	6·97
	Hand Picked in 1951	4·94	7·13
Neblongs	Machine Picked in 1952		7·06
	Hand Picked in 1952		7·45
Old Longbrook	Machine Picked in 1951	6·52	
	Hand Picked in 1951	7·29	

N.B.—Method of picking refers to that adopted in the previous season.

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During the past few seasons we have been investigating whether the damage which is done by cutting the bine can be lessened by varying the date of cutting, i.e. is there any evidence that if the hops are picked early in the season, more or less damage is done than if they are left over until they are riper? We have carried out these experiments for several seasons now, and are continuing our investigations. The design of this particular experiment is shown in Figure 1, and the results obtained in the last season are shown in Figure 2.

Figure 2 shows:—

(1) That as far as hand picked hops are concerned there is no evidence that the date of picking in 1954 had any effect on the yield of the same hills in the *subsequent* season (1955).

(2) That as far as machine picked hops are concerned there is a marked reduction in yield in the subsequent season when the bines were cut early in 1954.

(3) An illustration of the loss associated with machine picking (represented by the gap between the two lines).

This question may be considered alongside another matter we are investigating—namely, the rate of ripening of the hop during the picking season. We used the same gardens for this investigation as that used for investigating the effect of date of cutting and obtained our information from the control hills shown in Figure 1. The results are illustrated in Figure 3, from which it will be seen that there is a marked loss of soft resin associated with picking, by whatever method, in the early part of the harvesting season. There is now considerable evidence that the picking of hops tends to start too early. The local man who decides when to start from the look of the hops, hates, of course, to run the risk of his over-ripe hops blowing away at the end of the season. But these experiments seem to confirm that this contingency is not nearly so likely to happen as the farmer fears, and that it is very uneconomic to start picking too early. There are, of course, factors other than the ripeness of the cone to be considered. Hop picking has to be squashed into a hectic few weeks, and this strains the capacity of the kilns for drying, the transport, the storage capacity etc., to breakdown point, besides involving heavy capital charges for machines which are only used for three or four weeks a year. Again, hand pickers prefer to come down early in September rather than remain in the colder damper days of late September or even early October.

Thus, the picking period is one that has to be finely adjusted, taking into account the loss of a valuable part of the crop if picking

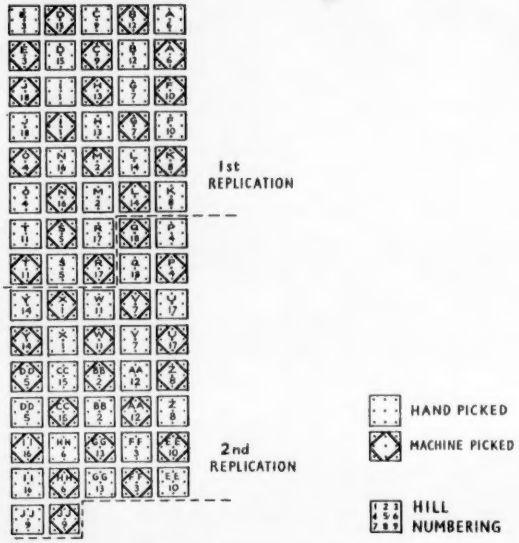

FIGURE 1. *Design of experiment to ascertain effect of date of bine cutting. Letters denote samples sent to laboratory; numbers relate to picking dates in* 1954 *(spread over* 6 *weeks). All hills were picked almost simultaneously in* 1955 *by hand.*

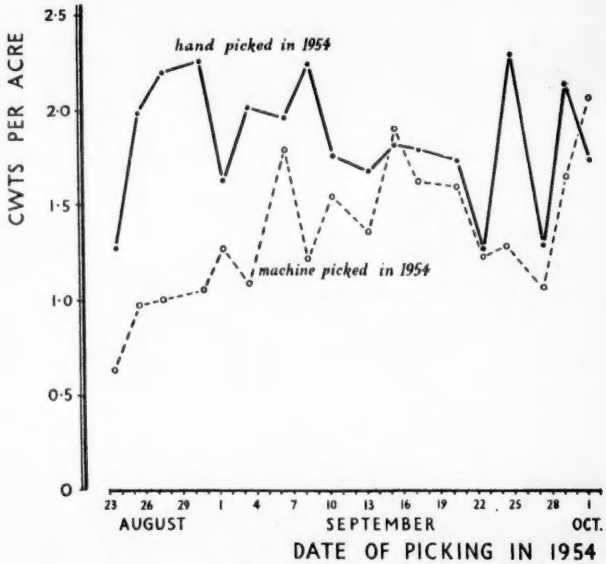

FIGURE 2. *Effect of date of cutting bine on yield in subsequent season. Yield of soft resins in* 1955. *(All hills were picked almost simultaneously in* 1955 *by hand.)*

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starts too early and weighing this against the cost of additional capital equipment if the season is shortened, and the cost of disgruntled pickers if the season is lengthened (even with machine picking, a considerable number of students etc., are employed cutting bines in the gardens, feeding the machines etc., and they obviously will not continue picking once their terms start). It looks however, as if the solution to the problem of optimum soft resin content during the picking season may also solve the other problem of the best time for bine cutting from the point of view of subsequent yield.

We will now turn to other experiments involving the use of different statistical techniques.

Firstly, some work has been done, and still is being done on the effect of grassing down hops. The arable usually surrounding hops involves repeated cultivation—a good hop garden is amazingly free of weeds—and it is felt that possibly by grassing down, the labour costs might be considerably cut. In addition there might be supplementary advantages; for instance, in a dry season hops under grass might remain better irrigated, grassing down might reduce the spread of some of the diseases to which the hop is prone. Similar experiments have been carried out by Wye College but you might like to see the results of one of the experiments we are doing.

TABLE II

GRASSING DOWN HOPS

	Grass	Arable
Weight of Green Hops per hill (ozs)	75·7	97·3
Yield of Dried Hops per hill at 10% Moisture (ozs)	15·69	19·24
Moisture Content of Green Hops (%)	80·73	82·24
Soft Resins at 10% Moisture (%)	8·23	7·96
Yield of Resins (cwts per acre)	0·87	1·04

You will see that there are indications that earlier experiments, indicating a reduced yield from grassed down areas, are likely to be confirmed.

Secondly, at great expense a small pilot experiment has been done with Krilium, that "wonder substance" which, according to an article I read in an American magazine, was going to "cure the world's starvation in a year." This experiment was planted on a normal randomized block basis and the results we obtained in 1955

when the hops first started yielding properly are shown in Table III.

TABLE III

KRILIUM

	Treated Plots	Control Plots
Green Weight (ozs per hill)	122·9	120·3
Dried Weight (grammes per hill)	25·2	25·6
S.R. at 10% Moisture	9·2	8·8

As far as yield is concerned there is no indication of any increase; the situation may, of course, change as time goes on and the hops mature. There is a small (but statistically insignificant) increase in soft resins.

Next you might be interested in some work done using a different statistical technique, namely, correlation.

Mr. Gray, who is Manager of our Bodiam hop farms, observed that there appeared to be a connection between the weight of actual hops picked off the bine and the weight of that bine. He had an idea that this might be a one to two relationship. Over a number of years now, covering a great many gardens, we have investigated this. The technique has been to define certain hills for sampling, and to put into the garden concerned, our special picking gang. This special picking gang consists of local worthies, chosen for their care and exactitude. The hills to be picked are marked and the gang goes in, cuts the full bine, weighs it and then picks the hops off, taking great care to avoid any extraneous matter, leaf, strig, and string etc. The hops are then weighed green. Figure 4 illustrates the results for one season.

A very good relationship appears to have been established with very little variability about the regression. We have investigated this for several years in several gardens, and we have found a regression of the form:

Weight of Hops = ·4265. Weight of Bine +1·730.

We have, however, found that there is some evidence that while the correlation remains high in each season, there is a seasonal effect that alters the regression. This means that the slope of the line is a seasonal factor, and because of this, if one wishes to establish with any accuracy the hop/bine ratio for a particular season, one would need to estimate the relationship anew. On the other hand, the difference between the regression coefficient is not very great, and were one using figures to give one a rough and ready guide as to the expected yield of green hops, one would probably be pretty safe in

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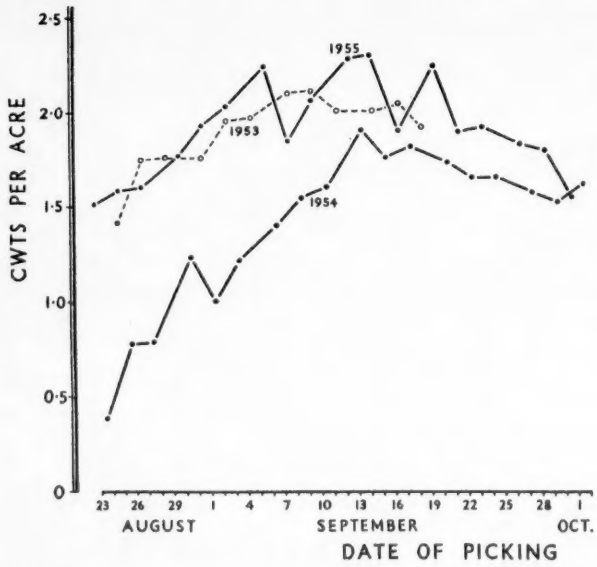

FIGURE 3. *Development of soft resins (yield of soft resins at 10 per cent moisture).*

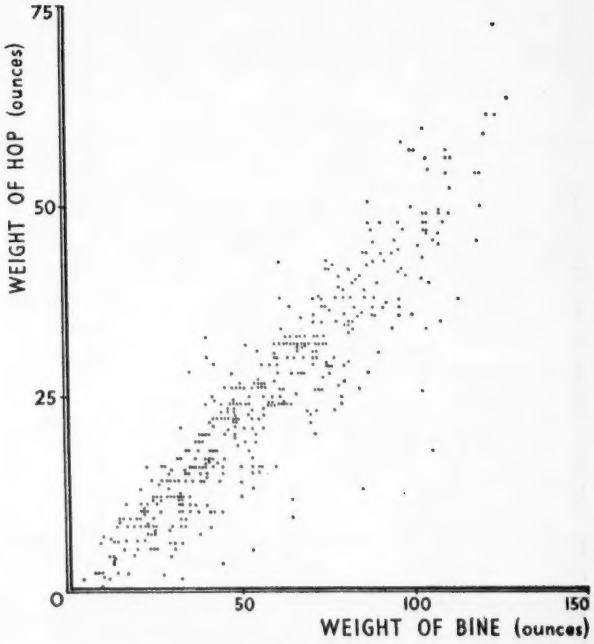

FIGURE 4. *Hop bine ratio*, 1954.

using the regression I have quoted. This relationship between the weight of ripe hops and the bine can be very useful in predicting the yield of particular gardens; it is also interesting as such a high correlation does not exist with some other natural products.

Another use of correlation and regression techniques can be illustrated by experiments carried out on the yield of individual hills. We have investigated the yield from particular hills over a number of seasons. The same type of sampling and picking techniques have been adopted to those already discussed, and we have found quite a high correlation between the yield in any one year from a particular hill and the yield from the same hill in the previous year, and also in the year before that. Now this result is one that could have important repercussions, because if it can be demonstrated incontrovertibly that a poor yielding hill is likely to remain a poor yielding hill, then it will obviously affect our grubbing policy.

Further experiments are planned to try and isolate the cause. It could be:

(1) A genetic factor: indicating that particular plants are innately weak, in which case we would probably do best to grub them very early on in their life.

(2) An environmental effect, that is to say, particular hills are so positioned that they get the full force of the wind, or are less likely to receive as good attention from the people training the hops, or are likely to have been crushed by the cultivator etc. This cause seems to be unlikely, as the distribution of poorly yielding hills over a garden appears to be quite random.

(3) A soil effect. However, the soil analyses we have carried out do not indicate that this is likely to be the main cause, and of course it would be very peculiar if manurial treatments for particular hills remained constant over several seasons.

In what I have said I have covered a few of the experiments we are doing now or have carried out in the past, in order to give you, as I said at the beginning, examples of a few of the simpler statistical techniques that have been applied to hop growing and picking.

In connection with our hop farms, we, as a Department, have to advise upon the design of experiments, and have analysed the results of experiments on the effect of systematic materials and insecticides; on the height of wire-work; on methods of stringing; on the spread of diseases; on kilning techniques; on drying in general; on sampling (and hops are extraordinarily difficult to sample). We have been involved in experiments to investigate the dung producing qualities of cows; we have worked on figures of subjective measurements of quality; we have advised on the errors

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involved in the various laboratory analyses; on pricing mechanisms; on irrigation experiments, etc. Work for the hop farms covers about one fifth of the time of my Department.

ACKNOWLEDGEMENTS

I would like to thank Mr. J. F. Brown, who is our brewer in charge of the hop farms, and Mr. C. J. Gray, our Manager at Bodiam, for the advice and help they have given me. In addition, I would like to thank the Directors of Messrs. Arthur Guinness Son & Co. (Park Royal) Ltd., for permission to read this paper, and the members of my own Department who have helped me.

INTRA-CITY TRAILER MOVEMENT

by

RAM VASWANI*

Introduction

SEVERAL APPLICATIONS of the simulation technique have been made in recent years for testing the effectiveness of different management policies for complex non-military systems, and interest in this area appears to be growing. Thus Cushen,[3] DeCarlo,[4] and Rowe and Jackson[15] have discussed the possibilities of the simulation technique for studying industrial and agricultural production problems. Berkowitz has discussed the application of graphical delay simulators for analysing air traffic problems.[2] Gerlough and Mathewson have pointed out the applicability of the simulation technique for the analysis of highway traffic problems.[6]

Among the applications of the simulated game approach to actual systems problems, mention might be made of the following: Scheduling and maintenance of United Airlines' aircraft;[1] analysis of queueing problems for aircraft landing at La Guardia Airport;[12] analysis of return to base problem in air traffic control (currently in progress at the Aviation Psychology Laboratory, The Ohio State University); analysis of requirements for truck dock facilities for a department store warehouse in Chicago;[16] study of signal timing for controlling individual highway intersections, considering factors such as road width, right-turning vehicles, composition of vehicular traffic, etc., carried out at the Road Research Laboratory in

* Operations Research Group, Engineering Experiment Station, The Ohio State University.

England;[8, 20] machine shop production scheduling problem studied at General Electric Company's jet aircraft plant in Evendale, Ohio.[14]

The purpose of this paper is to outline a method of attack for analysing a problem arising in the railroad industry. Specifically, the problem involves scheduling the movement of inbound and outbound freight trailers between the terminal and consignees' and shippers' loading and unloading docks.

The problem falls essentially within the domain of the queueing theory which has not been sufficiently developed to take care of complex problems of the type discussed in this paper. Hence, assumptions have to be made to make it manageable by the Monte Carlo technique.

The Problem

The outline of the method will be illustrated by reference to the case of a fictitious company, the Graybound Motor Freight Company, which is making a study of a new road-cum-rail freight service it proposes to offer to its clients. This new service consists of soliciting full trailer loads of freight from shippers and driving these on rubber into the freight terminal. After each trailer is placed individually on a railroad flat car, a train load of the latter will be hauled on rail to the destination freight terminal where each trailer will be driven individually on rubber by a motor tractor for delivery to the consignee. In the railroad industry, this is known as the piggyback service.[21]

The sales department of the Graybound Motor Freight Company has forecast that the volume of traffic will justify running a whole train between two given cities. On arrival at the destination terminal, trailers will be removed from flat cars and placed by means of a special purpose tractor, called the *spotter*, on a nearby parking area.

The problem is to answer questions such as the following:

1. How many spotters should be used?

2. How many tractors should be used?

3. Should spotters and tractors begin work at the same time (which may be expected to result in idle tractor time at the beginning of the day) or should the tractors start work after some trailers have already been placed and, if so, how long after?

4. Should tractors be assigned to trailers at random or should the consignees be classified?

5. If classified, should short hauls or long hauls be made first?

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Data Required

The following information is required and is assumed to be known:

1. The expected number of trailers n on an inbound train.
2. The expected time of arrival of train daily.
3. The location of the terminal, and the distance and travelling time between it and the potential customers' docks.
4. The probability of a consignee and/or shipper being located in any particular section of the city.
5. The probability p that an outbound trailer will be available in a section of the city for pick-up in the morning while inbound trailers are being delivered: (p might in fact vary from one section of the city to another and also vary for different hours of the morning).
6. The latest time by which the loaded trailers must reach the freight terminal to catch the outbound train.
7. Time required for removing trailers from an inbound freight train and placing them on the terminal parking area and the time required for the reverse movement for outbound trailers.

Policy Formulation

The technique suggested is how to formulate and test alternative policies. For instance, policy 1 could involve our having 2 spotters, 30 tractors, with a lag time of one hour; policy 2 could involve our having 3 spotters, 40 tractors, with a lag time of two hours, and so on.

Using the Monte Carlo technique,[10, 11] for each run we would draw a random sample of n from our distribution of location of potential business and this would give us the location where each one of n trailers will have to be hauled. Letting the total number of tractors which will return with loaded trailers $= m$, we can draw a random sample of m from our population of locations of potential shippers and this will give us the locations of shippers of m loaded trailers.

For each section of the city, we could also draw a random sample with probability p and on this basis decide which sections will have outbound trailers available in the morning. Let these sections be

$$j = 1, \ldots, m.$$

Optimal Assignment

The problem now becomes one of optimal assignment: assigning m tractors to m loaded outbound trailers so as to minimize the total

distance of haulage and therefore the time the tractors are kept busy. This is a special type of linear programming problem with the following model:—

Let,

t_{ij} = time that tractor i takes to go from its present location, and haul loaded trailer in section j to the freight terminal ($i, j = 1, \ldots, m$).

$$x_{ij} = \begin{cases} 1, \text{ if tractor } i \text{ is assigned to trailer in section } j, \\ 0, \text{ otherwise.} \end{cases}$$

The problem is to find values of x_{ij}'s such that

$$\sum_{i=1}^{m} x_{ij} = \sum_{j=1}^{m} x_{ij} = 1 \tag{1}$$

$$\sum_{i,j} t_{ij} x_{ij} = \text{Minimum} \tag{2}$$

Flood's technique for solving optimal assignment problems could be used for solving this problem.[5, 19] The answer to the problem will tell us the time when each tractor will return to the freight terminal and be available for making fresh deliveries if necessary.

Simulation technique could also be used to analyse simultaneously the problem involved in the making up of an outbound train of loaded or unloaded trailers, and of moving empties from consignees' to shippers' docks or the terminal.

If we make many Monte Carlo runs like this, for each alternative policy tested, we could compute the "expectation" in terms of a measure of effectiveness, e.g., the time of the day when the last trailer is delivered to the consignee's unloading dock, or the time of the day when the last loaded trailer is hauled back to the terminal from the shipper's dock, or the average of all delivery times, etc.

Variance of this expectation for each policy could also be computed. If these variances turn out to be compatible, the policies could be compared for significance of differences, either by the standard analysis of variance test[17] or by Tukey's method of easy comparison.[18] This is justified on the ground that for large random and independent samples, the sampling distribution of the means is approximately normally distributed even though the population is not normally distributed.[7]

Correlation of samples of different alternative policies is recommended for reducing the number of Monte Carlo runs for each policy.[9]

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Limitation of the Technique

The technique provides a basis for ranking, in the order of preference, only those alternative policies which are actually tested (*see* section on *Policy Formulation*). Thus only the policy that appears best amongst those tested could be determined. This is analogous to finding the local optimum as against finding the absolute optimum. The technique does not give an indication of whether or not a policy which if tested would have come out as the absolute optimal has been left out of consideration. Thus the best judgement should be exercised to see that all policies that are likely to be optimal are analysed.

The technique although accurate is not precise.[10]

ACKNOWLEDGEMENT

The author is indebted to Lawrence Friedmens of the Case Institute of Technology and to R. E. Beckwith, Purdue University, for suggestions on the original version of this paper.

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[21] ZIMMER, CHUCK, Renovation on Rails, *The Ohio State Engineer*, April 1956; **39** (5).

ABSTRACTS AND REVIEWS

An Optimum Value Solution to an Architect's Problem.

PROF. Z. TEROL. (In Spanish).
Trabajos de Estadistica, **6** (3).*

Solution of service planning for users of a building; for the general case with density functions in place of proportions. Typical, detailed.

Proposed method for Calculation by Simplex method of Hitchcock's Transportation Problem.

J. DE LASALA. (In Spanish).
*As above.**

Railway empty-car problem, based on Simplex and Rocossa's methods. Demonstration of choice of initial and succeeding bases for quick and easy solution, with method in degenerate cases. Detailed.

Traffic Forecast in relation to Highway Planning.

Road Abstracts, **23** (5).

Quotes F. M. VAN VEEM, Wegen, 1955; **29** (8) 214–24 in Dutch with English summary.

Formulated on hypothesis that traffic distribution will bear relation to frequency of length of trip and number of vehicles in each starting place. Methods of calculation, test of formulae and example.

* See note on page 98.

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Physiological Consequences of Work: Sickness of Executives.
M. A. BETHOUART. (In French).
L'Etude de Travail, April, 1956; **62**, p. 39–45.

Analysis of deaths among managers and top executives: study of death rate by age of executives and general death-rate, of frequency distribution of illness in various vocational groups. Efficiency not by attendance but by biological faculties. Detailed.

Industrial Amortization: relation of Laws of Individual Plant item Depreciation on the Capital Charge of a large Enterprise.
M. BOITEUX. (In French).
Annales des Mines, March, 1956; 3.*

Choice of amortization on cost or final replacement. Rhythmical depreciation on progressive ageing. Depreciation of a unit, and relation to plant group. Detailed.

Methods of calculating Profitability of Capital Equipment.
E. VENTURA AND J. LESOURNE. (In French).
*Reference as above.**

Definition of true profitability; mathematical complexity and usual short-cuts; safe short-cuts with tables for rapid approximations. Detailed, interesting: reference to Hogben's "Chance and Choice".

Cost Reduction.
Gee & Co. (Publishers) Ltd., 27, Basinghall Street, E.C.2, for the Institute of Cost and Works Accountants.

The usual meaty sense the Institute provides, to the point and in Industry's own idiom—an excellent phrase-book for the scientist in strange country.

Future Trends. (I.C.W.A. Midland Regional Conference) April, 1956.*

Good "user appreciation" of formal O.R. in non-technical terms by W. G. Jones, Fisons Ltd.

Organisation of Operational Research.
STAFFORD BEER.

Science, Statistics, and Operational Research.
N. T. J. BAILEY.

Linear Programming.
F. H. HAHN.

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* See note on page 98.

Operational Research and Queuing Problems.
R. S. GANDER.

Probability and Operational Research.
R. BENSON.

An Application of Work Sampling to Operational Research.
L. H. C. TIPPETT.
Research, 1956; **9** (5), (6), (7), (8), (9) and (10) respectively.

Stafford Beer's introductory article is a refreshing review of the field, summing-up the position as the scientist should see it. He shows that the individual situation is not typical, and the solution of a specific case will be valid for the observed situation alone.

Stressing the need for seeing conditions as they are, not as they might be, he shows the need of the broad approach and the opportunity of a whole new solution. Soundly though modestly, he gives a quasi-solution to the problem of organization.

Each of the succeeding papers follows a pattern of method, example and references. The series covers the ground well.

Linear Programming (in two parts).
Metal Working Production. 16 *Dec.* 1955; *p.* 2128–33.
30 *Dec.* 1955; *p.* 2215–19.

I. Definition: simple applications: description of use: short bibliography.

II. Case History Application (The Glacier Metal Co. Ltd.). Based on Simplex: demonstration of short cuts for the case: detailed presentation. (To be published in greater detail in *Applied Statistics*).

Making the Best Use of Scattered Storage Depots.
A. H. SCHAAFSMA.
Metal Working Production. 20 *July*, 1956; *p.* 1101–05.

Linear programming case history: iteration method with short proof.

Businessman's Question and Answer on Operational Research; Linear Programming.
V. C. HARE AND R. VASWANI.
Business, August, 1956; *p.* 99–104.

Title gives fair description; value of grouped data; example and references.

*The above abstracts are contributed by ORbit (Operational Research) Ltd. Copies of publications marked with an asterisk can be obtained on loan from that firm at Empire House, St. Martin's-le-Grand, London, E.C.*1, *at a charge of* 1*s.* 6*d. for postage.*

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Bolletino del Centro per la Ricerca Operativa, 1 (1).
Centro per la Ricerca Operativa, Milan. 42 *pp. Lire* 600 *per issue, Lire* 2,000 *per annum.*

We welcome the first number of this quarterly bulletin. In his editorial comment Prof. Francesco Brambilla sets out clearly the nature and scope of O.R. and discusses the mathematical methods available. A paper by B. Giardina on the optimization of production sequences in the manufacture of various machine parts made on different machines describes graphical and numerical methods of solution. The classical method of linear programming is not applicable in this particular case as a well defined sequence of operations must be adhered to. A paper by L. Castoldi on the distribution of intervals between consecutive events in Poissonian and non-Poissonian stocastic processes discusses some of the more general aspects of Poisson's law and the associated physical problem of the ionization of particles.

Space is given to a carefully abridged version of J. G. Wardrop's paper on "The Capacity of Roads" (*Operat. Res. Quart., Mar.* 1954; **5** (1), 14–24). Readers will find the bibliography of O.R. literature published in the U.K., the U.S.A., Germany and Spain useful for reference purposes.

A feature of the bulletin is the inclusion at appendix of a number of prepared "cut out" index cards giving in each case the author's name, the title of the paper, and an abstract. The cards are designed for alphabetical filing.

The Bulletin (which is undated) is attractively presented and augurs well for the future numbers to come.

M. D. J. B.

International Journal of Abstracts on Statistical Methods in Industry.
International Statistical Institute.

This Journal, first published in the Spring of 1954, was the result of the setting-up in 1951 of an official "I.S.I. Committee on Statistics in Industry and Technology". The Committee, which wished to foster mutual contact and exchange of information between industrial statisticians in all parts of the world, considered that an effective means to this end would be the setting-up of an international bibliography reviewing the most important articles published in the journals of the world. Since the scope of industrial statistics has widened greatly in the years since the war there is a real need of a service of this sort. The editorial organization is truly international, under an American general editor, with regional editors in U.S., Spain, Italy, France, Scandinavia, Netherlands, Japan, Great

Britain, Western Germany and India. The general editor is Dr. G. I. Butterbaugh.

Abstracts are either in French or in English (predominantly English). The abstracts are short, seldom exceeding 150 words, and printed on one side of thin cardboard. One page can be cut into three parts 3 in. × 5 in. each, so that the resulting cards can be arranged in a card index system.

The subject matter covers the whole field of technological application of statistics and also market research, sales administration, operational research, and personnel statistical work. Work of a theoretical statistical nature is included.

So far three issues a year have been made at the rate of about 100 abstracts in each. The index is divided as follows: (1) authors, (2) journals abstracted, (3) subject, the latter being divided into (A) Statistical, (B) Technological, (C) Management.

After two years it can be said that this non-commercial undertaking is performing a vital service with efficiency. The annual subscription is $5.00 (postage included).

W. N. J.

Quality Control and Applied Statistics—Abstracting Service.
Interscience Publishers, 250 Fifth Avenue, New York, and London.

This is a new Abstract Service, published in New York, although an address is given in London so that the fees can presumably be paid in sterling by subscribers in this country. The publicity leaflet states that the Service "abstracts all new contributions—new methodology or new applications of known methodology—devoting one or two pages, sometimes more, to each abstract. This permits giving the significant features, with equations, tables, figures and illustrations where necessary, so that it should usually be unnecessary to consult the original paper. This first issue, starting June, 1956, contains 37 abstracts and there are to be twelve issues, totalling approximately 1,000 pages annually. Subscription price: $60.00 per year."

The list of 300–400 journals which are scanned is impressive. It includes all the major mathematical and statistical journals in Europe, Australia, India and Japan, as well as specialist journals of a technical character (chemical engineering, plastics, etc.) and journals dealing with economics.

This Abstracting Service has one positive and welcome virtue. Each abstract is self-contained, a single loose-leaf or a loose-leaf folder. The individual abstracts can, therefore, be easily filed, and cross-indexing is not difficult from that point on, according to the

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system suggested by the Service or any other convenient to the user. Each abstract is arranged as: Title, Authors, Journal, Purpose, Summary, and Results.

Whether the editors are really successful in their main purpose, which is to make each abstract so informatory that it is scarcely necessary to consult the original paper, is doubtful, but it is true that in the specimens which have been sent to review, the condensation has been remarkably successful and it has usually been found possible to include the reproduction of the principal formulae and diagrams from the original article. For example, the abstract of a paper on "Automatic calculation and recording of maximum, minimum, and average values" from *Control Engineering*, February, 1956, includes a diagram of the basic circuit, a Brush Recorder Chart, and a Maximum-Minimum Detector Chart; and "Graphical Method of arbitrary area under the bivariate normal probability function" from J.O.R.S.A., February, 1956, includes the working out of the differential equations, and the "bivariate mapping ruler" is illustrated to scale.

The 37 articles in the first issue are presumably the first effort, and it would be unfair to make criticisms too drastic. It can be said, however, that there is a bias towards the more elementary aspects of Statistical Quality Control, and that this issue would be much more useful to engineers and others seeking for enlightenment in this direction than to mathematicians looking for references in their particular field of activity. More stress might be laid upon examples of the use of new statistical models and techniques. These criticisms may be met in the future, but in the meantime, this seems to be a useful Abstracting Service.

A. W. S.

OPERATIONAL RESEARCH IN FRANCE

La Société Française de Recherche Opérationelle has been formed with the object of furthering methods and applications of operational research in France. It is intended to systematize and centralize existing documentation on the subject, and to promote O.R. by all possible means, including the publication of a journal. For these purposes a permanent secretariat is envisaged. As well as full participating members, it is intended to admit as corresponding members persons whose interest is limited to the literary and advisory activities of the Société.

The Société's address is 40, Rue du Colisée, Paris 8e.

LONDON COMPUTER GROUP

THE London Computer Group held its first convention at the Bonnington Hotel, London, on 4 July, and was addressed by Professor B. C. Learke and G. J. Mills. The Group was formed on 19 April, with Dudley W. Hooper, M.A., A.C.A., as chairman and A. J. Bray, M.A., A.C.A., and J. P. Hough, A.C.A., 19a Coleman Street, London, E.C.2, as honorary secretaries.

A correspondent writes:

"For all but a few initiates the subject of computers must still carry an aura of mystery. One knows one ought to have a use for them; just what perhaps remains less certain. In keeping with this atmosphere was the receipt early this year of an anonymous stencil—addressed to nobody in particular, coming from nobody in particular—summoning all the faithful to a meeting of the London Computer Group.

"A packed meeting, duly held on 19 April, voted itself by a show of hands unopposed, if not with unanimity, into being as the London Computer Group and elected a committee consisting of the lady and gentlemen sitting in prepared positions.

"The objects of the group have since been released and are described as follows: 'Prime object of the Group is to provide, through small study groups, the means whereby individuals with different professional interests may examine a common problem in computer use. For example, the developing concept of integrated data processing supposes complete understanding, between, say, the production engineer, the accountant, the statistician and the computer specialist, of each other's needs from a single processing of original data.

" 'The results of such examination of problems in specific fields carried out by each group will be circulated so that a fund of experience may be available to all members. The first groups will cover such subjects as the application of computers to production control, a study of input/output mechanisms and preparation of prime data, the impact of electronic data processing on management control and administrative organization, to name three only of the fifteen groups so far planned.

" 'The Group aims to encourage the spread of knowledge in the computer field and to act as a focal point for the exchange of information. It is thought that understanding of an "integrated" approach can best be achieved by an organization providing facilities for individuals each with a specialized knowledge and interest to meet and discuss their common problems, scientist

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alongside manager, accountant alongside engineer, linguist alongside actuary.'

"Finally 'The Group is pledged to identify itself' (whatever that may mean) 'with any national association of individuals that may be formed in the future with similar objects'.

"Any attempt to broaden the understanding of the true potential of computers deserves encouragement. Most professional bodies concerned are already active in the field and one must doubt whether the subject has yet reached the stage when groups of strangers can profitably discuss together, each in his own jargon, general problems without a common, specific, personal, interest. However, the list of officers and members of the committee shows a heavy preponderance of accountants so it may be that, in the event, their deliberations will be less catholic than is proposed; they will probably be none the worse for that."

Evening Courses in London

The Northampton Polytechnic, London, E.C.1, announces that the following evening courses will be held during the coming autumn session. All lectures start at 6.30 p.m.

Theory of Probability for Application in Operational Research and Statistics (G. A. Garreau); 12 lectures from 27 September.

Linear Programming (S. Vajda); six lectures from 4 October.

Theory of Games (S. Vajda); five lectures from 15 November.

CORRESPONDENCE

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MANAGEMENT SCIENCE

Sir,

I was interested to see R. S. Gander's letter in your last issue. The idea of asking the British Standards Institution to publish a report defining the science of management, or defining operational research fills me with horror. It is far too early for anyone to seek to define the boundaries of operational research, and in any event I cannot see how this will lead to the extension or unification of the subject. It is surely far better to continue to allow the subject to grow naturally as an interaction between the operational research scientists on the one hand, and their environment on the other. Even so I doubt whether we shall ever reach the stage of being able to define where operational research ends, and another science begins. This is not surprising, and is indeed the general case with every scientific subject.

The other point which Mr. Gander mentioned was the proportion of members in our own Society, and in O.R.S.A., who come from universities and research institutes. I was careful to use the phrase "research institute", and not "research association" in my earlier article, since I was trying to classify those who had the time and opportunity to do fundamental research. My impression is that the opportunities for this are significantly greater in operational research groups in the American research institutes, than in the British research associations. (I shall now, Sir, order my sack-cloth suit and the latest fashion in ashes in anticipation of the indignant replies you will get from our own research associations.) However, working on this criterion, our own Society has five per cent of its members in British universities, and two per cent in American universities. This compares unfavourably with Mr. Gander's estimate of the O.R.S.A. university and research institute proportion of twenty-six per cent, and indicates what I still maintain is a weakness in current British operational research.

Yours truly,

B. H. P. RIVETT,

Hon. Secretary.

Operational Research Society,

London, S.W.1

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OPERATIONAL RESEARCH SOCIETY

THE Annual Report for the year ending 31 July, 1956, was presented at the Society's Annual General Meeting on 24 October. Among the points made were the following:

"Although the usefulness and standing of a Scientific Society are not necessarily reflected by total membership, it is encouraging to note our continued expansion over the past years. The following table shows the numbers of full members and associates since the formation of the Society in 1953, as at 31 July each year:

	1953	1954	1955	1956
Members	78	101	148	187
Associates	—	—	15	60
Total	78	101	163	247

"Six meetings were held during the year: October 1955, *The Assessment of the Potential Output of a Slabbing Mill*, H. G. Jones (Steel Company of Wales); November 1955, *Some Experiments in the Growing of Hops*, Miss S. V. Cunliffe (Guinness, Ltd.); December 1955, *What are Operational Research Methods?*, N. Jessop (Courtaulds, Ltd.); January 1956, *Quantification of New Inventions*, The Earl of Halsbury (National Research Development Corporation); February 1956, *Subjective Factors in the Sorting of Paper*, Dr. H. F. Rance and I. F. Hendry (Wiggins Teape Group Research Organization); March 1956 Open, Meeting on *The Influence of Data Processing Machines on Operations*, Dr. B. V. Bowden (Manchester College of Technology), D. G. Owen (BISRA), Dr. S. H. Hollingdale (R.A.E.).

"The Society co-operated with Northampton Polytechnic in organizing a series of lectures on Operational Research. These seemed to meet a widely felt need. About 150 people registered for the course and the average attendance was over 100.

"It gives the Committee pleasure to record the close links that are being established on an informal basis with Operational Research Societies in other countries. These are finding a definite expression in the first International Operational Research Conference which will be held at Oxford University in September 1957. This is being organized by the Society in conjunction with the Operations Research Society of America and the Institute of Management Sciences (U.S.A.).

"For many years past the subscription has remained constant at 25s. per annum. Rising costs have forced the Committee to decide, with great reluctance, to recommend that this be raised to 2 gns. per annum as from 1 August, 1957. The endorsement of the

Society to this recommendation will be sought at the Annual General Meeting to be held on 24 October, 1956. (The meeting endorsed the recommendation.)

"The Committee wish to express their thanks to Dr. T. E. Easterfield, the retiring member, for all he has done for the Society during his period on the Committee."

R. S. Gander was elected to the Committee in the place of Dr. T. E. Easterfield, who retired under rule. The following officers were re-elected: *Hon. Secretary*, B. H. P. Rivett; *Hon. Treasurer*, E. C. Williams; *Hon. Auditor*, R. G. Stansfield.

The Editors of the OPERATIONAL RESEARCH QUARTERLY reported that paid circulation now exceeded 1,000 copies for the first time. In the year under review the four issues contained 150 pages against 166 in the previous 12 months. This in part reflected the smaller number of abstracts published (a decline which new arrangements from September onwards were designed to arrest), but to a greater extent an insufficiency of papers and articles submitted for publication.

The circulation figures at 30 June, 1956, with the corresponding figures for 1955 in brackets, were:

Members of the O.R. Society	243 (155)
Subscribers in Great Britain	267 (274)
U.S.A.	279 (255)
Other countries	228 (170)
	1,017 (854)

The Society's warm thanks were again due to the QUARTERLY's honorary subscription agent in North America, Dr. Peter B. Myers, whose efficiency and generous enthusiasm had in this as in previous years been a tower of strength.

HONORARY MEMBERSHIP SECRETARY

The Committee have appointed B. D. HANKIN to be Honorary Membership Secretary. He will deal with all enquiries and applications to join the Society.

Address: B. D. Hankin, Office of the Director of Research, London Transport, 55, Broadway, London, S.W.1.

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A PROBLEM IN SELECTING RANDOM NUMBERS

The Design Principles of "Ernie"

by

S. WOOD*

IN lotteries or sweepstakes, the prize winners may be identified by numbers on tickets held, or may depend on the results of a sporting event. In those of the first class, the order in which the tickets are sold is significant in so far as the purchaser may have a choice of the serial number of the ticket he buys albeit only within the limits of the book of tickets from which the purchase is being made. The books of tickets having been distributed independently prior to the sale, the distribution of tickets among purchasers is largely but not completely random and this random element in the ticket holding would normally suffice to offset any small bias which might exist in the selection of the winning numbers.

Whereas in most lotteries a ticket qualifies for one prize draw only, a special problem arises in connection with Premium Savings Bonds because each bond (after being held for six months) qualifies for a succession of prize draws at monthly intervals. If a would-be purchaser thought he detected a bias in the prize-winning numbers in successive draws, he could not in practice take advantage of it since he would have to accept whatever bond was next due to be sold at that selling point at which he chose to make his purchase or go elsewhere. (There are some 30,000 selling points.) But a holder of bonds who suspected a bias would be influenced in deciding whether to leave his bond current indefinitely or to encash it. Quite apart therefore from satisfying an obvious public requirement for a fair selection of prize winners there is this additional reason for a prize-winning selection system more free from bias than those normally used.

Prize-winning tickets in a sweepstake may be drawn by the haphazard selection of counterfoils after they have been thoroughly mixed in a drum. The well-founded prejudices against this system are further increased when there are successive draws, if the selected counterfoils at one draw have to be replaced in the drum (ready for the next draw) after they have acquired new physical characteristics simply as a result of being handled. (They will acquire a certain amount of grease, crumpling, etc., from handling, so differentiating them physically from the counterfoils not selected.)

* General Post Office.

At the time this paper is being written some £40 million worth of Premium Bonds have been sold. At the first draw therefore there may be about 20,000 prize winning bonds. (Prize money in the first draw will be equivalent to 2 per cent of the value of eligible bonds and the prizes are of such value that there are 247 for each £10,000 of prize money.) At any draw some of the serial numbers selected as prize winners will relate to bonds not eligible for prizes (because of the qualifying period), to encashed bonds, or to bonds issued to selling points but not yet sold. It follows therefore that, of the selected numbers, a big proportion will be discarded, and to allow for this, at the first draw, for instance, the number of selections may have to be of the order of at least 30,000/40,000. No existing mechanical system for producing random numbers is known which would give such an output in a reasonable time.

Clearly then the method of selecting prize-winning Premium Bonds has to be something quite different from that so far used in national lotteries or sweepstakes in other countries. The size of the draw, the eligibility of bonds for selection in successive draws and the possibility of a bond holder encashing his holding at any time all introduce special difficulties in the design of the system.

A method of selecting the winning bonds is therefore being developed by the Post Office Engineering Department Research Branch, who have designed the electronic random number indicator equipment "ERNIE" which (i) will be free from the elements of bias to be found in such a rudimentary system as described in the third paragraph above, and (ii) for operational efficiency as well as manifest impartiality does not need the intervention of an operator between the start of the prize selection and the printing of the list of selected bond numbers.

The Research Branch has been using for some time a source of random digits based on an electric discharge through a gas. In the gas tube, the gas molecules move about in a random manner. When a potential is applied, the electrons passing to the anode are subject to collision with these gas molecules and arrive therefore in varying numbers from instant to instant giving rise to random variations of current which can be amplified and detected. By the use of a "slicing" valve it is possible to neglect the preponderant number of changes of current of low amplitude and to "square up" the remainder into pulses of constant amplitude to operate a counter.

The number of pulses received by the counter in any one period varies over wide limits but the distribution curve of the numbers is single-humped and, because the mode is large relative to 10 (the slicing level is such that about 3,000 squared up pulses are recorded

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per second), the number of pulses counted terminates in the digits 0, 1, 2, . . . 9 with almost equal frequency. (The distribution curve would need to be rectangular, of course, for the frequencies to be precisely equal.) While in theory there are minor differences in frequency when the counter starts from zero each time, in practice there has been no evidence of bias from this cause. But if the counter is not restored to zero each time it is stopped and the new digits generated are merely added, the theoretical differences in frequency of occurrences are eliminated.

The machine was designed to give an output satisfactory for the purpose for which it is required. To confirm this the digits have been subjected to the kind of tests described by Kendall and Babington Smith (*J. Royal Statistical Society, Part* 1, 1948, pages 147–166). In addition to the four standard tests described in paragraph 24 of their paper, another test on the lines of the permutation test in paragraph 38, developed in America under the title "Coupon Collector's Test", has been applied. The results have come up to expectation.

"ERNIE" is made up of nine sets of these sources in the form of electronic noise generators driving nine counters which can be stopped simultaneously at fixed intervals decided by a master pulse-generating unit. Each counter relates to one digit so that the nine separate digits of the number defining a winning bond are generated independently. Thus there should be no correlation between them. Moreover, each generator with its associated counter is provided in duplicate and the results from the two counters are added in combining units having ten outputs. In this way, any bias arising in any one of the counters due to a faulty condition in an electronic tube will be safeguarded by the addition to the output of the counter of digits generated at random by its opposite number in the duplicate set. Each time the counters are stopped, the outputs from the nine combining units are transferred to primary storage equipment from which they, and the fixed codes—e.g., figure shift, letter shift, line feed, carriage return and space—needed for the printing of the numbers are assembled, scanned and fed to the printing mechanism together with a sequential draw number. The first two digits of the prize winning bond numbers will be used also to identify the part of the office where the corresponding bond counterfoil is filed and equipment associated with "ERNIE" will route the information to a teleprinter in that part of the filing section. After the digits have been recorded the noise generators are allowed to drive the counters again so that they move on from the position in which they were last stopped.

Since "ERNIE" will generate numbers anywhere in the range of zero to $10^8 - 1$ (the ninth digit is an alphabetical prefix) and the bonds bearing the higher numbers may not have been distributed to the selling points, arrangements have been incorporated in the primary number storage for the suppression of random numbers generated above prescribed values (a separate value for each alpha prefix digit). This suppressor will be adjusted immediately before each draw to correspond to the highest serial numbers in each series of bonds issued. This arrangement goes some way to reducing the selection of numbers to be discarded for the reason indicated in the fourth paragraph above.

When "ERNIE" is completed, the tests referred to will be applied for some weeks by the Post Office Engineering Department Research Branch to whom the writer expresses his appreciation for advice on the technical matters in this paper. The results of the tests will no doubt be published in due course for the benefit of the statistical world.

TIME-STUDY AND STATISTICS

by

A. W. SWAN, B.A.Sc., A.M.I.Mech.E.*

Introduction

TIME-STUDY by stop-watch was invented in the early part of this century as a part of what was called Scientific Management and used principally as a basis for the piece-work and bonus payment systems of Taylor, Gantt, and others. Whether by stop-watch, special film techniques, or other mechanisms, time-study has remained and still is primarily a function linked with payment for work done. Motion and method study, invented by Gilbreth, have remained, until fairly recently, separate from time-study.

Present-day industrial statistics may be defined as the statistics of the small sample. As it is seldom possible to have available for analysis the total possible amount of numerical information on costs, weights, times, etc., it is usually necessary to examine only a sample of this numerical information. Statistical methods provide ways of assessing from such samples the properties of the "populations" from which they are drawn within defined limits of probability. A basic assumption in industrial statistics is that no series of measurements remain exactly at a given value but vary about that value.

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The variation may be narrow or wide, symmetrical, "skew" or rectangular, but it always exists.

Industrial statistics as defined above is a relatively new arrival, and it has only been during and since the war that there has been any real impact of this type of thinking on industry. The pioneers of time-study were aware that time-studies were samples of information and that there was variability in the time-study man, the operation studied and the operator. They made suggestions to deal with variability, however, not as the present-day statisticians would by examining a complete situation but by selection of "reasonable" or "representative" operators, jobs and machines to study.

The term "statistics" seems to conjure up to many industrialists techniques that are hard to understand and complicated to use, yet the statistical point of view is essentially a simple one. It may be summarized as: (1) measurements are always variable so that a single measurement does not represent the whole situation, (2) since there is variability one must assess not only a mean value but the extent of the variability, (3) more than one measurement is necessary to make such an assessment, (4) one can estimate total effects with reasonable accuracy from what may be considered to be a small sample of such measurements.

The statistical device of plotting numerical information in the form of distributions is extremely simple and can be used without any training in statistical theory. In connection with time-study it provides a very valuable method of analysis.

Examples of Statistical Methods applied to Time-study

The following section gives some examples of the application of statistical thinking to time-studies, using principally the plotting of results as distributions and occasionally the use of more advanced techniques, particularly correlation.

(*a*) *Time-studies without Rating*

The following examples are concerned with time-studies made in connection with heavy machines such as steel-rolling mills, cranes, etc., where "rating" has not been used.

Case 1

It was desired to condense, if possible, over 65 groups of "expected tons per hour" for a certain steel cogging mill (rolls ingots into blooms) so that clerical work could be simplified. By plotting actual results taken from works records over six months in the form of distribution curves as shown in Figure 1, it became possible to use

6 groups instead of the 65 normally used. It was found, for instance, as will be seen in the diagram, that there was no material difference between the average rolling rates for the different sizes of billets and that a single grouping would suffice.

The visual indications of the distribution curves were checked statistically before the groupings were adopted.

The above case does not illustrate the application of statistical methods directly to time-study but to recorded actual performances in which every time-study man and cost accountant is interested, the method used being a quick and simple yet accurate way of arriving at the time element in standard costs.

Case 2

In order to gauge mill capacity, time-studies were made in the roughing and finishing mills in a steel works which produced rails. Since the run-out bed on which the rails are finally cooled would only take rails up to a certain length it was necessary in the case of a certain light-weight rail to roll the ingot into a bloom, saw the bloom in two immediately after rolling and proceed to roll each half of the bloom successively into a rail short enough to be accommodated on the cooling bed. It was found in connection with this particular rolling that the manipulation time prior to the first pass of the first "half" rail showed a very long rectangular distribution whereas the corresponding manipulation time for the second "half" rail showed a distribution with a slight tail, as shown in Figure 2. As there was no apparent reason why there should be such a difference between the first and second "half" rails investigation was made of a number of rollings in chronological order. It was found that the ingots were being rolled at irregular intervals, but that once an ingot was rolled into a bloom it was then promptly sawn and the resulting two halves rolled. Thus the interval between the arrival of the first half was irregular and showed as a rectangular distribution, while the interval between halves was regular.

It was deduced that a rectangular distribution of times for industrial processes may be an indication of trouble at the process immediately prior to that being studied, a deduction borne out in subsequent analyses.

Case 3

A study of a bottleneck in a rail bank (out-going store) in which rails are accepted and despatched, included time-studies on two sets of cranes which in sequence handled the rails from their arrival from the rail-strengthening department to their departure on British

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Railways wagons. The first crane deals with rails as they arrive in sequence on a conveyor, one at a time, from the rail straightening department. When there is a "lift" of five rails, the crane picks them up, carries them along a gantry and drops some half-way and the remainder at the end of the run. The crane then returns along the gantry and picks up another "lift". In theory, five rails should arrive during the delivery run of the crane, so that when it returns a new "lift" is ready. There may, however, be a waiting period. Thus the operation of the crane may be divided into:—

1. Lift.
2. Travel.
3. Unload.
4. Return Travel.
5. Wait.

The distributions are illustrated in Figure 3, and the deductions are:—

1. Lift and Unload.—Here we find symmetrical narrow distributions indicating uniform practice and good work.

2. Travel and Return.—In this case the distribution is two-humped (bi-modal) and symmetrical, with a reasonably narrow base as before. This indicates good uniform work, the two modes being the result of the fact that some of the rails are dropped half-way along the gantry and the remainder at the end of the gantry.

3. Wait.—This is another rectangular distribution, and the deduction is the same as that in Case 1, indicating chaotic conditions at the previous operation, in this case rail straightening.

The study of the two cranes working on a single gantry at the despatch stage at the same rail bank was interesting in other ways. It had been thought that the purchase of a new crane might be necessary, but the distributions from the time-studies, coupled with chronological analysis of the same data, showed that by making various improvements in planning the order of events, the efficiency of the crane usage would improve to such an extent that this would not be necessary. Examination of the time-study information in distribution and chronological form also showed clearly the interesting fact that the team of two cranemen had worked out an efficient way of operating so that they did not get in each other's way and formed an excellent team.

(*b*) *Time-studies with Rating*

Early time-study was a straightforward timing of machine or manual operations, broken into convenient elements. From such

studies a representative time was assessed for the operation and used as a basis for the piece rate or bonus time. "Rating" was introduced in the 20's and early 30's and is now almost universally used in conjunction with time-study. There are a number of systems but a typical one is to assume that the normal speed of working, sometimes equated to walking at four miles per hour, is assessed at an "80" rating. If the operator is working harder than is assumed normal, the rating goes up to 90, 100, and occasionally 110; if the effort is less than normal it is dropped to 70, 60, and even 50. This time is then adjusted according to the rating; thus if the rating is 90, and the actual time is 9 seconds, the adjusted time will be

$$9 \times \frac{90}{80} = 10 \text{ seconds.}$$

The justification put forward for rating time-studies is that to take "actual" times only as a basis for setting rates of payment would be an open invitation to the operator to work at less than his or her normal speed. Whether or not this opinion is valid, the difficulties in rating are many and among these difficulties the following can be indicated (a normal working rate of 80 being assumed for convenience).

1. The rating "80" is usually taken to be equal to the effort in walking at the rate of four miles per hour; but it is one thing to equate rating to the speed of walking and another to equate rating to the great variety of manual and manual-machine operations, bearing in mind the variability of:—

(*a*) Job conditions (machines, etc.).
(*b*) Training.
(*c*) Psychological conditions in the operator.
(*d*) Fatigue.
(*e*) Relation of actual speed to appearance of speed; an apparently slow operator may be working faster than one who seems to be working quickly and is in fact fussy.
(*f*) Relation of speed to quality. Speed, if increased beyond an optimum point, may lower quality.

2. Even assuming that a normal rate of working can be assessed, it is by no means easy to settle on the degree by which effort is faster or slower than the assumed normal. The scale of values for rating may be long or short, e.g., from 50 to 110, or from 60 to 100. The scale of values may be symmetrical or unsymmetrical about the standard (80), e.g., 60, 80, 100, or 50, 80, 100. The steps of the rating may be in 5's or 10's, e.g., 70, 75, 80, etc., or 70, 80, 90, etc.

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3. Time-study is normally taken by stop-watch to the nearest second or 1/100th minute, and nowadays on a continuous basis. Thus an operation divided into four elements is timed continuously, taking intermediate readings from the stop-watch as they occur. A four-element job might have successive stop-watch readings of 0, 23, 40, 48 and 60 seconds, giving 23 seconds for element A, 17 seconds for element B, 8 seconds for element C, and 12 seconds for element D.

The normal working unit is one second and there is thus a possible error in reading of $\pm \frac{1}{2}$ second, an appreciable factor. There may, however, be a balancing effect in consecutive elements. If element A above is actually $23\frac{1}{2}$ seconds and is read as 23 seconds, element B is actually $16\frac{1}{2}$ seconds but is read as 17 seconds.

4. If the element time is short, unless the rating is considerably higher or lower than the standard, the adjusted time will be very little different from the observed time. Thus if the observed time is 5 seconds and the rating is 85, the adjusted time is

$$5 \times \frac{85}{80} = 5{\cdot}3125 \text{ seconds.}$$

which, rounded off to the nearest second is still 5 seconds. The same effect is to be found to a lesser degree up to 8 seconds, for which the rating must be below 75 or above 85 to make a change of a full second from "observed" to "rated" time.

5. Rating is associated with a rounding-off error apart from the actual observed length of time. If the actual time of 5 seconds is rated at say, 60, the rated time will be

$$5 \times \frac{60}{80} = 3{\cdot}75 \text{ seconds.}$$

which will be rounded off to the nearest second at 4 seconds. The "rating" rounding-off error is thus

$$4 - 3{\cdot}75 \text{ seconds} = 0{\cdot}25 \text{ seconds or 6 per cent.}$$

On a longer study this might well be serious, especially when it is realized that a rate of payment depends upon the results.

6. The "easy number" effect may occur, that is a subconscious stressing of the round numbers in ratings, 60, 70, 80, 90, etc., coupled with a corresponding neglect of the in-between numbers, 65, 75, 85, etc. Adjustments are made for these round-off errors in Courtaulds' work study practice.

The above conditions are realized by all time-study practitioners but the application of statistical thinking to their solution is still

somewhat rare. Among the published works attention is particularly directed to a small publication by Martin Wiberg under the title *Work-Time Distribution* which cuts right to the heart of the matter.

Wiberg stresses that work-studies are always samples; they should be "representative" rather than "purposive"; the actual shapes of the distributions are very revealing in regard to degree of difficulty, stage of training, etc., and should, therefore, be used whenever possible.

Possible Deductions from Statistical Analysis

Before analysing some actual rated time-studies let us consider some of the deductions which may be made from the correlation-regression scatter diagrams for observed times and rated times $\left(\text{actual} \times \frac{\text{rating}}{80}\right)$.

The relationship between actual (observed), time, and the applied rating is somewhat unusual among industrial linkages in that either may be considered the dependent variable. As the time-study man may be considered to be primarily interested in the time corresponding to rating, time has been taken in this instance as the dependent variable and the reciprocal of the rating as the independent variable. In D. J. Desmond's paper the reciprocal of the rating, which he calls the reciprate, is the dependent variable.

Under ideal conditions one would expect that there would be a straight-line relationship between observed times and the reciprocal of the rating, the line being that from the origin "0" passing through the time for the "80" (normal) performance. This may be called the "ideal" line. The "actual" line tends to be steeper than the "ideal". As will be seen from Figure 4 when the "actual" line is steeper than the "ideal" the time-study man is using a compressed range of rating values, under-rating the faster times, e.g., 85 where the "ideal" suggests 100, and over-rating the slower times, e.g., 75 where the "ideal" suggests 60. The steeper the "actual" line in relation to the "ideal", the greater is this effect and the smaller will be the alterations by rating of actual times until in the extreme case, with a vertical "actual" line there is only one rating in use. This will mean that the distribution of rated times is precisely the same in shape as the distribution of observed times, at the same level if the rating is 80, moved upwards bodily if the rating is more than 80, and moved downwards bodily if the rating is less than 80.

The correlation scatter diagram yields further information through the value of r^2, the square of the correlation coefficient, and s the standard deviation of the scatter about the regression line which

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is the best fit to the cloud of points. r^2 and s are measures in the opposite direction (r^2 increases while s decreases), of consistency in rating. If r is so small as not to be significant in the statistical sense no definite regression line can be fitted to the points and there is, therefore, no traceable linkage between rating and observed times. With r significant but small, s is large indicating a considerable variation in the rating. Thus, in a single study under such conditions one might find a rating of 60 applied to times between 20 and 40 and a rating of 70 applied to times between 15 and 35, giving an overlap.

The variability in rating can be divided into components, that attributable to the personal variation of the time-study man, and that attributable to the variability in the difficulty of the job. If the mean time assessed at a rating of 80 under what are considered to be standard conditions is 10 seconds and the time-study man's personal error is assumed to be ± 1 second an 80 rating may be allotted to $10+1=11$ or $10-1=9$ seconds. If, however, we find an 80 rating given to a time of 15 seconds it is reasonable to assume that there is a "variability" or "difficulty" margin of between 4 and 6 seconds ($15-11=4$, or $15-9=6$ seconds). The greater is the "job variability" or "difficulty" component in the total variability the smaller will be the link between rating and observed times, until in the extreme condition rating will have no effect whatever in adjusting observed times. When this occurs the distribution of rated times will be almost exactly the same as the original distribution for observed times, with, in this case, no change of mean as may be possible with the "steepness" effect.

It follows from the above reasoning that if we compare time-studies of different operators on the same job by the same time-study man, variations in the "difficulty" variability will indicate differences in the difficulty experienced by the different operators in handling the job.

Figure 5 shows an imaginary time-study considerably over-simplified to illustrate the effects on the ratings, and through the ratings on the adjusted times, of "steepness" of the "actual" regression line in comparison with the "ideal" line. The observed times are the same for all four cases, giving skew distributions with a mean of 10 seconds.

In A the operator is considered to be working at about 80, in B at about 70. In A1 and B1 the ideal and the actual lines coincide, while in A2 and B2 the actual line is steeper than the ideal. It will be observed from the figure that for A1, in which the ideal and actual lines coincide the rated times form a symmetrical narrow-based

distribution, the mean being the same as that for the observed times, 10 seconds, since the average rating is 80 = normal. The ratings also form a symmetrical distribution covering the full range of available ratings. In A2 where the actual line is steeper than the ideal, the distribution for rated times approximates in shape to that for the observed time, with the mean still at 10 seconds as in A1. The distribution for ratings shows the effect of compression of the range of values used.

In B1 and B2 the effects are similar, except that the mean of the rated times is reduced from 10 = mean of the observed time to $10 \times \frac{70}{80} = 9$ seconds (to the nearest second).

Analysis of Actual Examples of Rated Time-studies

The following section analyses rated time-studies which have been supplied with the full co-operation and interest of Courtaulds' Work-Study Department. These studies are of manual operations which involve considerable dexterity and skill and which are usually short in element times. They were taken at a number of different mills in the ordinary course of work and not specially for the purpose of this paper. Standard jobs were taken except for one interesting case in which three women operators were studied on a difficult variation of a job which in the ordinary way is straightforward. The time-studies have been analysed as distribution curves of observed times, rated times (observed time × 80/rating), and ratings, together with correlations of observed times with ratings.

Case 1

Cleaning Returned Cone Tubes. Works A. (Figures 6, 7.)

This is a difficult and "finicky" hand operation.

1. *Observed Times.*—The distribution is rectangular, suggesting that the job was not only difficult but varied in difficulty and that the operator had not mastered the technique. Urgent need for method-study is indicated.

The deduction from the rectangular distribution of the actual times that method-study was required was proved to be correct. Unknown to the author of this paper, method-study has been carried out on this job since the time-study was made, resulting in suggestions for improved and simplified procedure which it is hoped will give better and more stable conditions.

2. *Rated Times.*—The distribution is rectangular, similar to that for actual times, indicating that the time-study man recognized that

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the operator was having a difficult time and considered that she was working consistently with standard or better than standard effort.

3 and 4. *Ratings.*—These are concentrated between 75 and 95 which tallies with the fact that the regression line is steep; the time-study man tended to compress his range of ratings.

In general, the rating had very little practical influence on the allowance for the job, since the rated time distribution is almost exactly that for the actual time.

Case 2

Cake Wrapping. Works B. (Figures 8, 9, 10.)

Stockinette to Former—two operators.

1. *Observed Times.*—The distributions are slightly "skew" for both operators with a mean of 6·4 seconds for Operation A, and 4·9 seconds for Operator B. The times are uniform within a narrow range, with only an occasional longer time presumably caused by slight difficulty.

2. *Rated Times.*—The distribution is almost identical with that for observed times. A contributory reason in this case for Operator A is that most of the times observed are 5 seconds and the ratings are 85. As mentioned earlier, for 5 seconds,

$$5 \times \frac{85}{80} = 5{\cdot}3 \text{ seconds},$$

which is rounded off to 5 seconds (no change from observed time); rating, therefore, makes no effective change in this instance. For Operator B, however, most of the ratings are at 90, just sufficient to alter the 4 and 5 seconds observed to 5 and 6 seconds rated. The distribution for rated times is, therefore, similar in shape to the observed with the mean moved upwards by about a second.

3. *Regression.*—Regression diagrams are not shown since r^2 is not significant or barely significant indicating that the link between rating and observed time is extremely weak, and that the "difficulty" margin is correspondingly high.

Case 3

Cake Wrapping. Works C. (Figure 11.)

Stockinette for Former.

This study is shown as an example of the "easy number effect", in this case a subconscious stress on the round numbers, 60, 70, 80, etc., with avoidance of the intermediate 65, 75, etc.

Case 4

Wrapping, Sealing and Packing 2½ *lb Cones.* Works A. (Figures 12, 13.)

This is an interesting example of a very high correlation between observed time and ratings. It will be observed from Figure 13 that r^2 has no less a value than 0·93 indicating that 93 per cent of the variation in the times is linked with rating, while $s = 0{\cdot}71$ a correspondingly low value. It will be observed also in this case from the distribution curve of ratings that there is no "easy number effect", the distributions being smooth from 75 to 95 with little evidence of preference for either 70, 80, 90, or 75, 85, 95.

Case 5

Coning 150/40 *Black.* Works C.

This was an intentional selection of a difficult job, in this case a variation on the standard job of coning. The main difficulty arises from the fact that the yarn being black is hard to see and, therefore, to handle.

Studies are shown of 3 out of 5 elements in the operation, for three women operators who have proved themselves in ordinary work to be experienced and conscientious and for whom, therefore, long element times can normally be attributed to difficulty rather than to slow working.

(*a*) *Lace Up.* (Figure 14.)

1. *Observed Times.*—From the narrow symmetrical distribution for operators A and C, it is evident that they have overcome difficulties and settled down to routine. Operator B has apparently almost reached this stage but has occasional difficulties.
2. *Rated Times.*—For all the operators distributions are similar to those for observed times. The average for operator B is slightly higher than for A and C, but the mode is about the same.
3. *Ratings.*—These are concentrated on 80 for A, spread from from 60 to 90 for B, and are concentrated from 70 to 80 for C.
4. *Regression.*—*r* is not significant for operators A and C and *r* is significant for operator B although not very high in value. There is the usual compression of ratings with the corresponding tendency to under-rate good performances and over-rate poor performances. The "difficulty" margin is thus much in evidence.

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(*b*) *Tie Knot.* (Figures 15, 16, 17 and 18.)

1. All three distributions for observed times tend to be rectangular, indicating instability of conditions. Presumably all three operators are still finding this a difficult job, but A and C are much further advanced in self-training to overcome the difficulties than B.
2. From the distributions for rated times and ratings it would appear that the time-study man has considered that A and C are working fairly consistently at just below and at standard effort, whereas B, in addition to having difficulty is not working quite so hard. As the average and mode for B's rated times are higher than for A and C, operator B is being treated rather more kindly.
3. Regression shows the usual characteristics of a short range of ratings.

(*c*) *Straighten Cake.* (Figure 19.)

Here the rectangular distribution shows that all three operators are in serious difficulty and have not yet discovered how to do the job to the best advantage. Rated times give the same average and mode for A, B, and C, and as in "Tie Knot", operators A and C are considered to be working in the 70 to 80 range whereas B is rated from 50 to 70.

Conclusion

The above paper has for practical reasons been confined to certain special aspects of the use of statistics in connexion with time-study. It has not, for instance, referred to the method of time-study devised by L. H. C. Tippett originally called "Snap Reading", and now "Ratio Delay" or "Activity Sampling", a method founded on statistical reasoning, nor to the valuable help afforded by statistics in examining the problem of machine interference. References to both of these subjects are given in the bibliography. The principal stress has been on the fact that the simplest of all statistical tools, the distribution curve, can be of immense assistance to the practical time-study man, providing him with much key information which he would be otherwise unable to obtain.

It would not be wise to draw too many hard and fast conclusions from the time-studies of manual operators which are the basis of the second half of this paper as they form only a small sample of a population of time-studies. Nevertheless certain points emerge. The first is that benefits can be obtained by time-study men who use

ratings if they will plot distributions of observed times, rated times, and ratings, and also draw a regression diagram and obtain a line either by the usual formulae or by short-cut inspection methods. From the distribution curves they will be able to obtain a good indication of the uniformity or otherwise of the job which they would not be able to obtain from the tabulated results, and if they are making studies of more than one operator a ready means will be provided of assessing the relative mastery of the job of the different operators. The regression diagram will show the extent of the "variability" or "difficulty" factor and the degree of "steepness" of the actual line. The former will be a useful check on the general effectiveness of rating as an adjusting factor in the particular time-study under consideration, and the latter will show the extent to which the time-study men are compressing the range of ratings they are using. It is suggested that the best way to obtain the "80 (or other standard) rating time" is to use the value shown on the regression line rather than take the mean or mode from the distribution curve of rated times as is a current practice.

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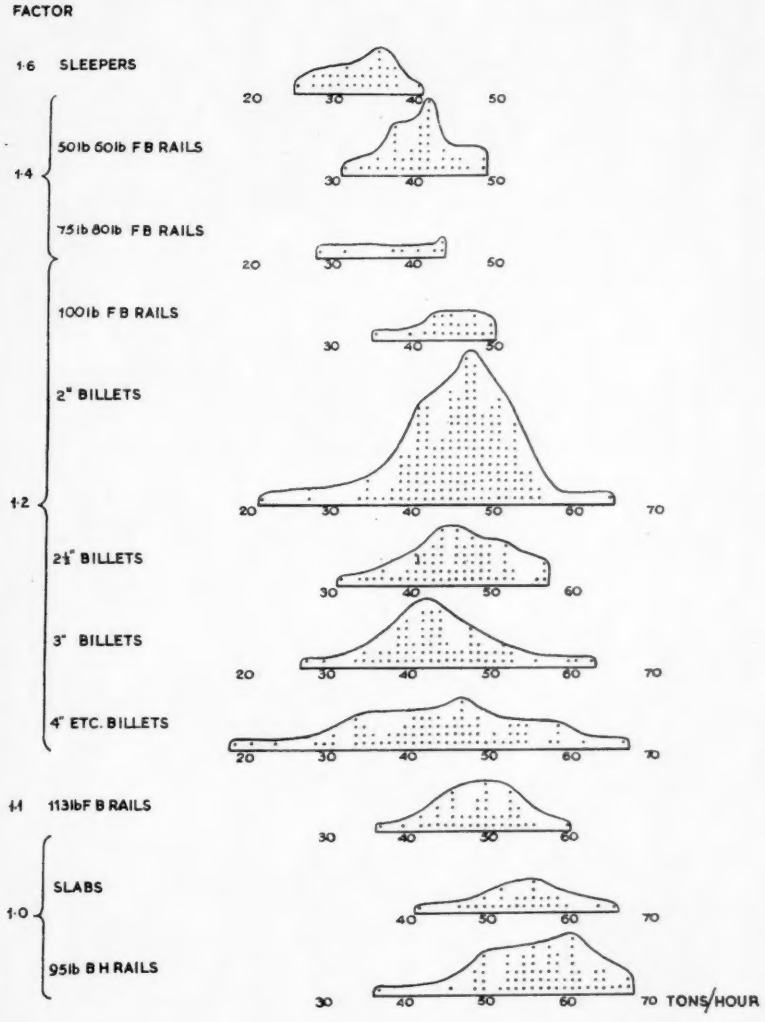


FIGURE 1. *Net rolling rates, tons per hour.*

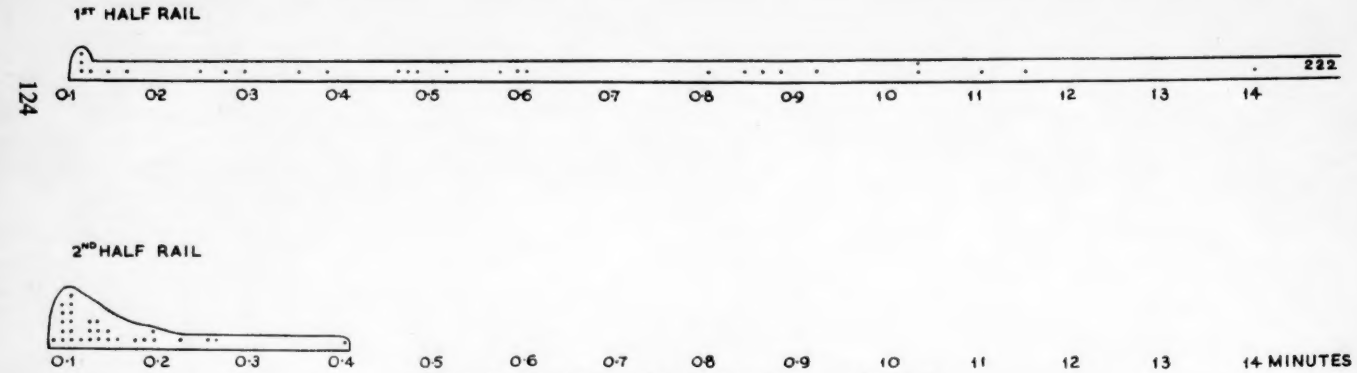
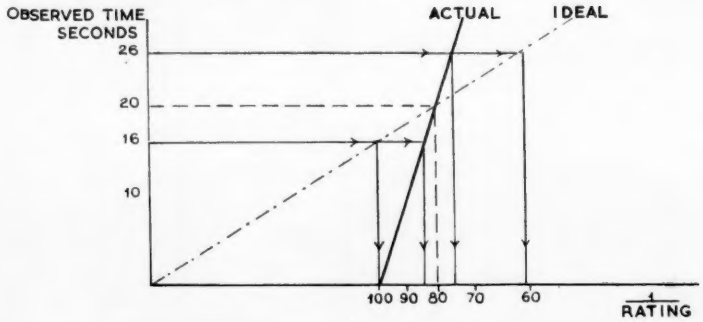


FIGURE 2. *Manipulation time for first and second half rails.*



20 SECONDS AT 80
16 SECONDS RATED AT 100 ON IDEAL, AT 85 ON ACTUAL
26 " " " 60 " " " 75 " "

FIGURE 4. *Relation of ideal to actual rating.*

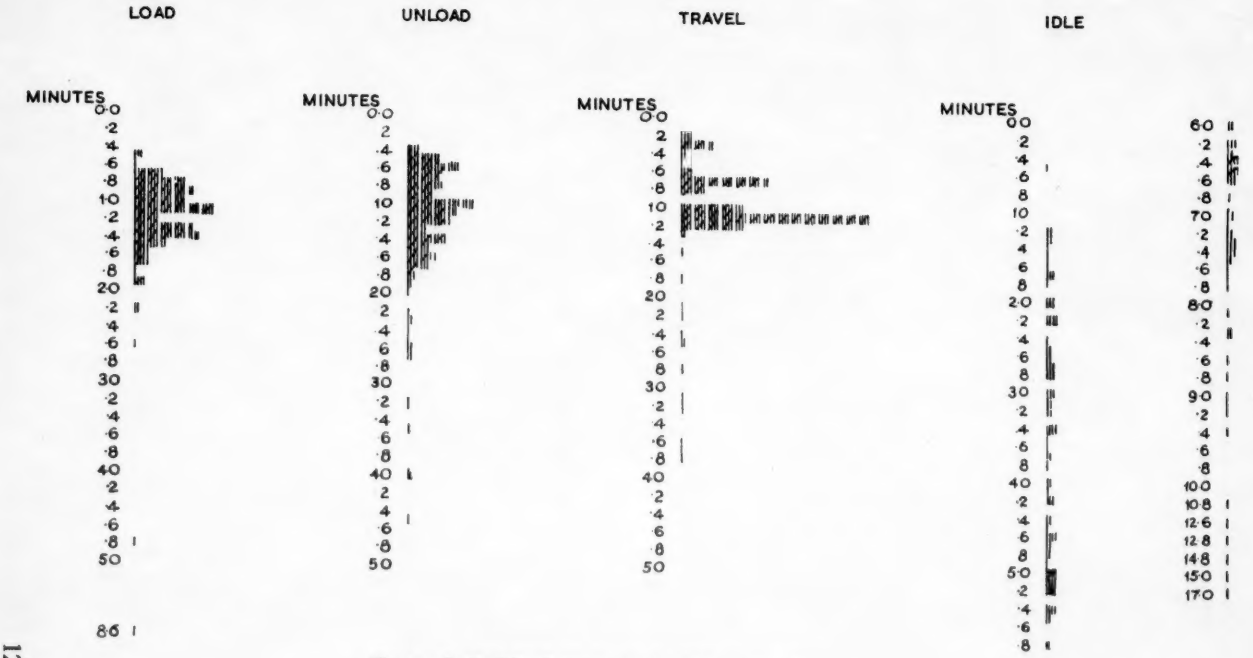


FIGURE 3. *Rail bank crane, 95 lb. rails one week.*

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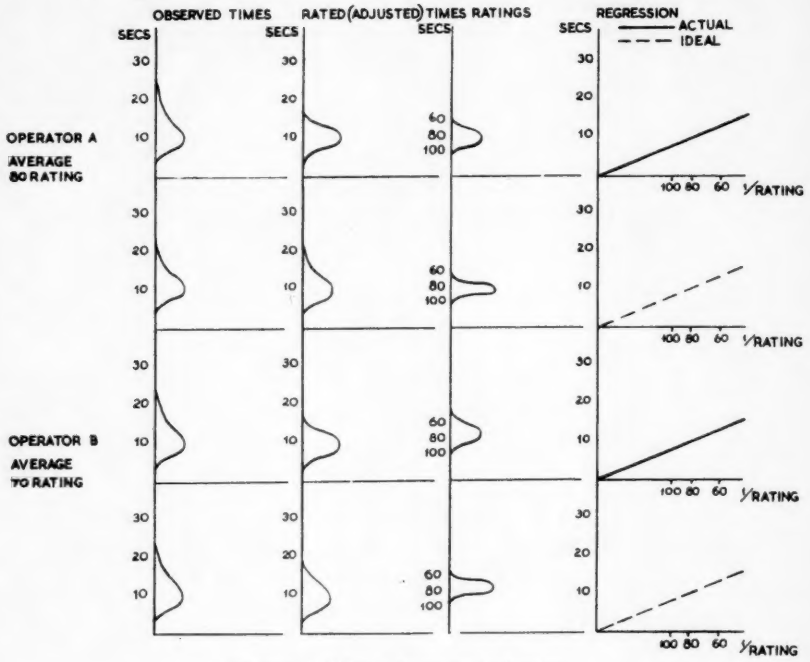

FIGURE 5. *Effect of steepness of rating.*

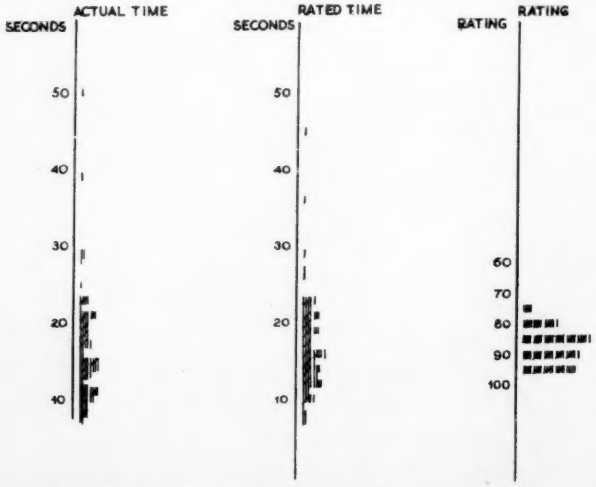

FIGURE 6. *Works A, cleaning returned cone tubes.*

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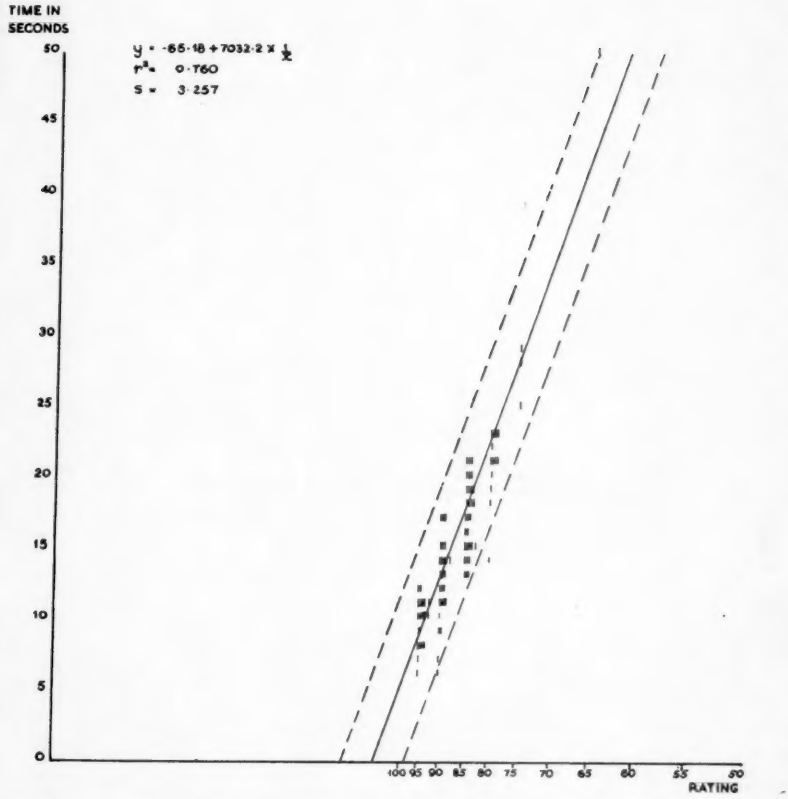

FIGURE 7. *Works A, cleaning returned cone tubes.*

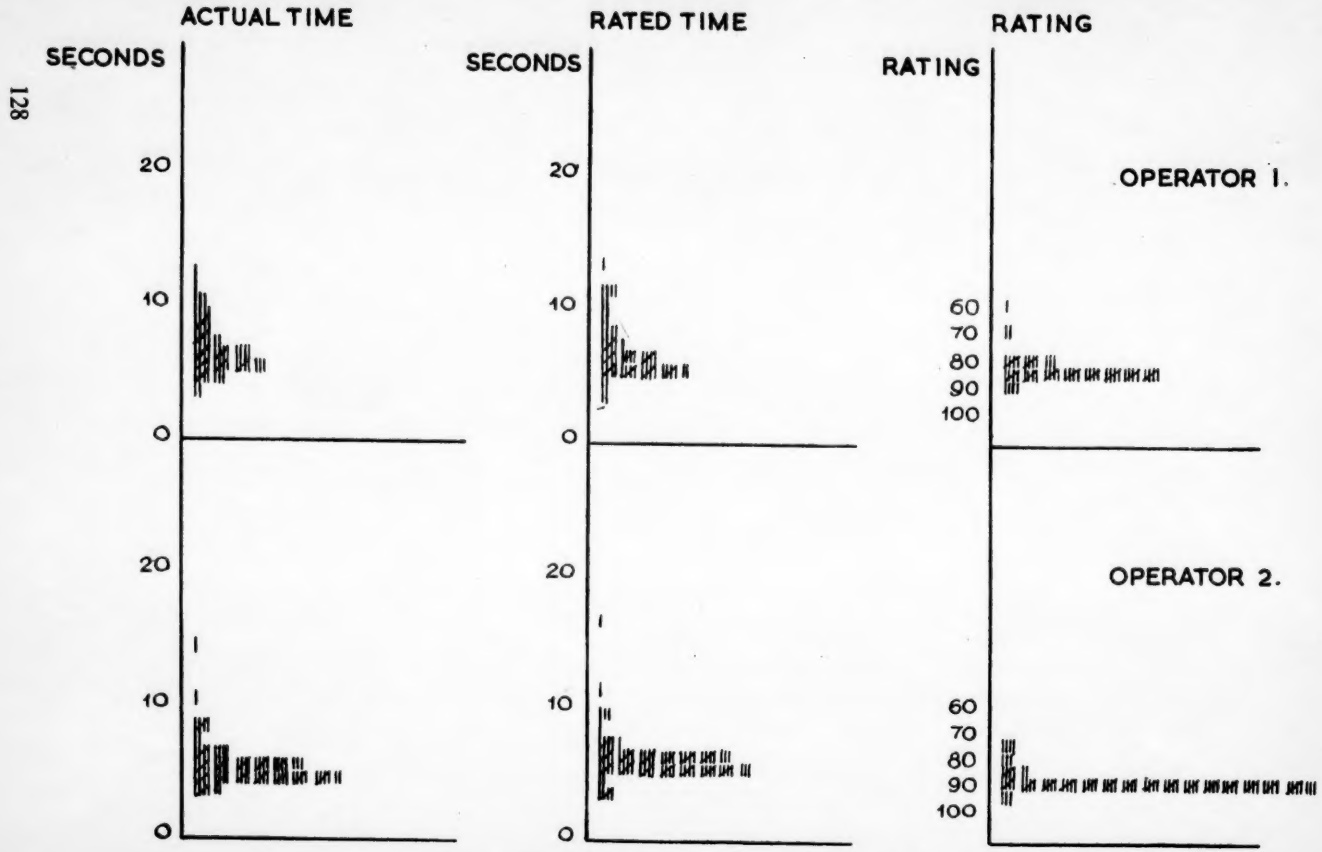

FIGURE 8. *Works B, cake wrapping, stockinette to former.*

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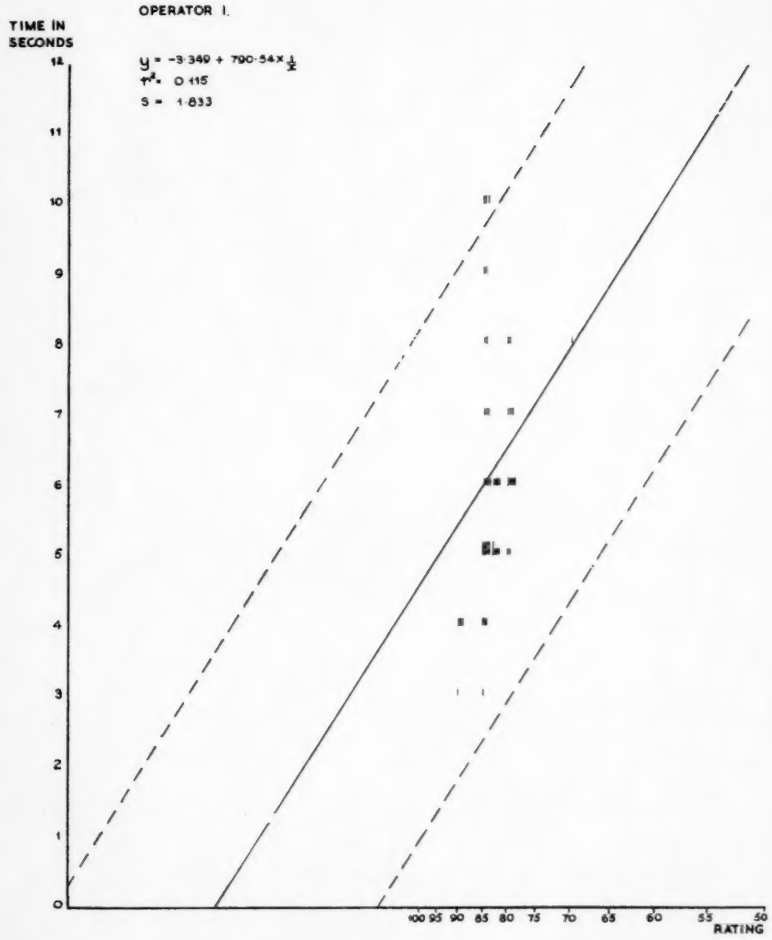

FIGURE 9. *Works B, cake wrapping, stockinette to former.*

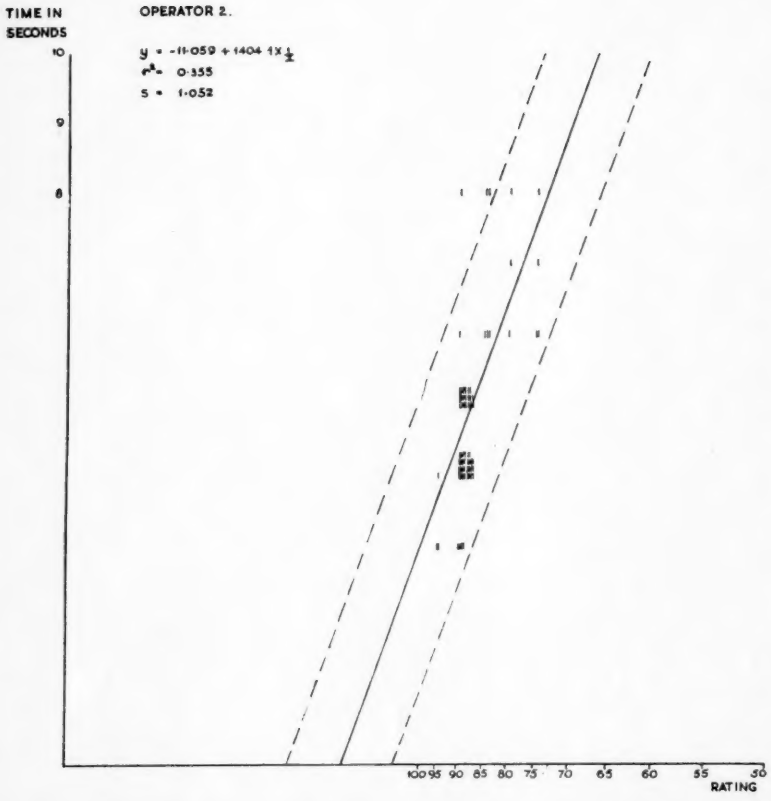

FIGURE 10. *Works B, cake wrapping, stockinette to former.*

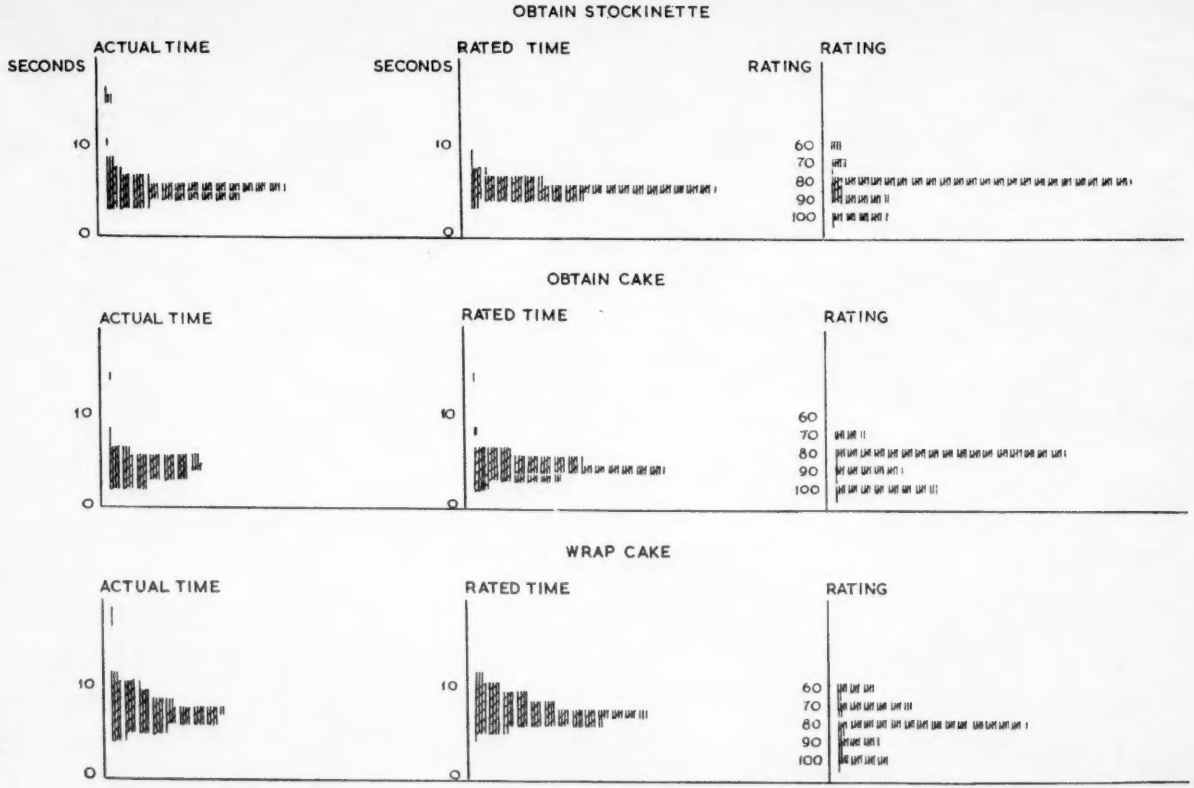

FIGURE 11. *Works C. coning, lace up.*

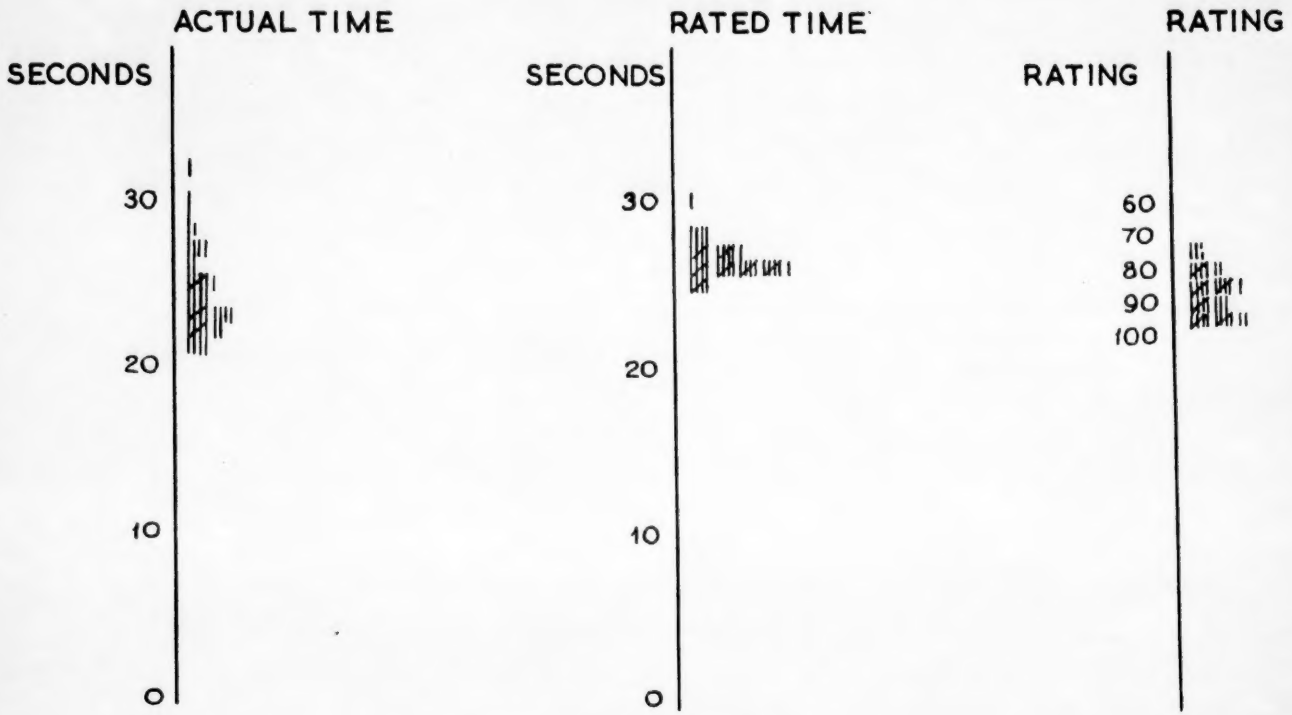

FIGURE 12. *Works C, cake wrapping.*

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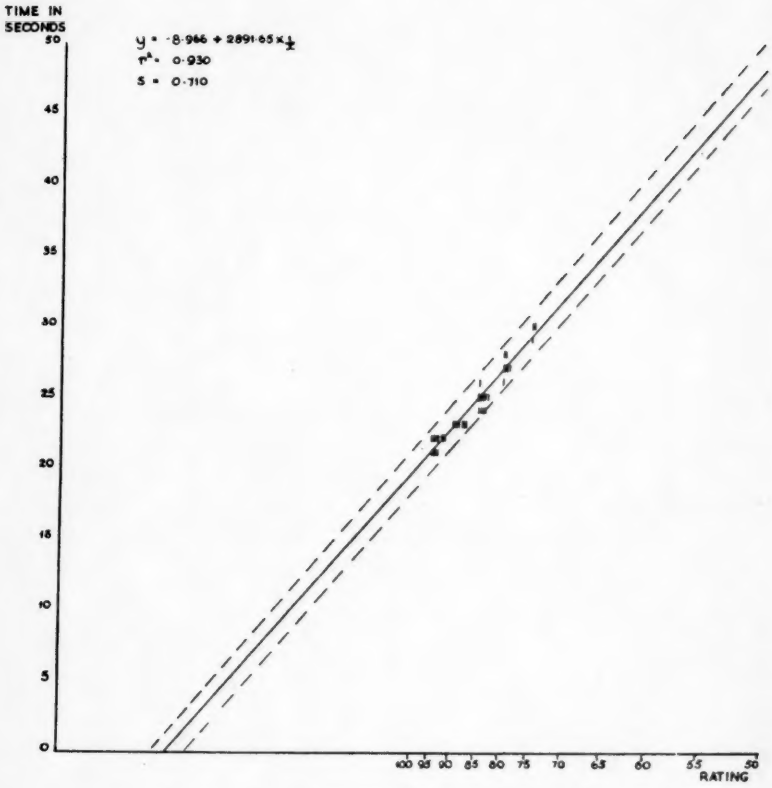

Figure 13. *Works A, wrapping, sealing.*

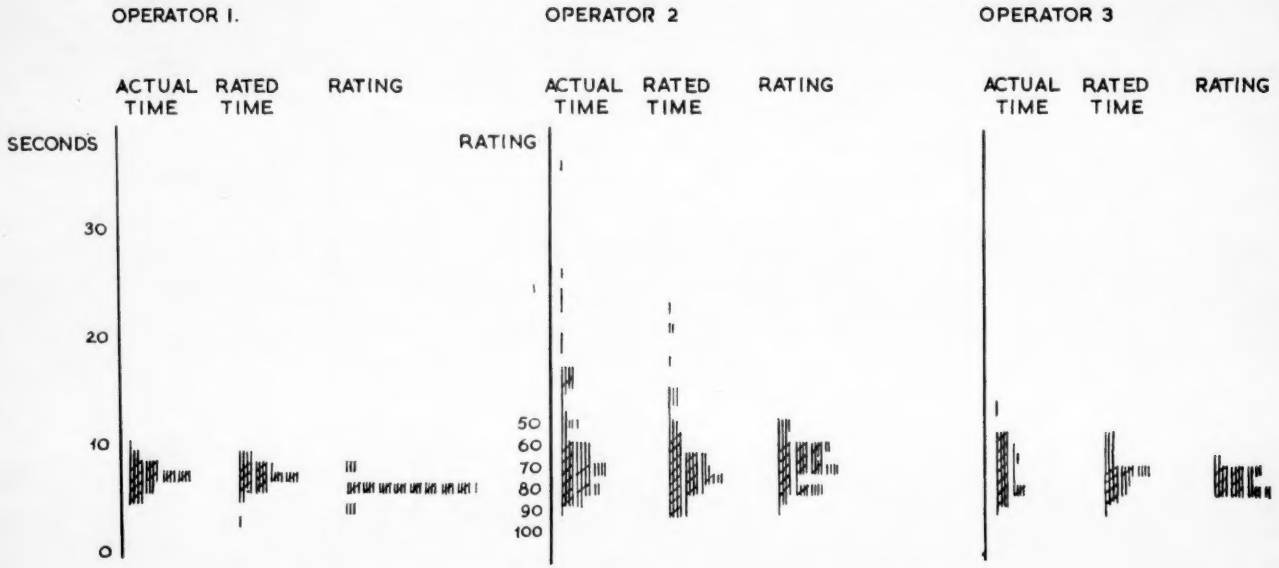

FIGURE 14. *Works A, wrapping, sealing.*

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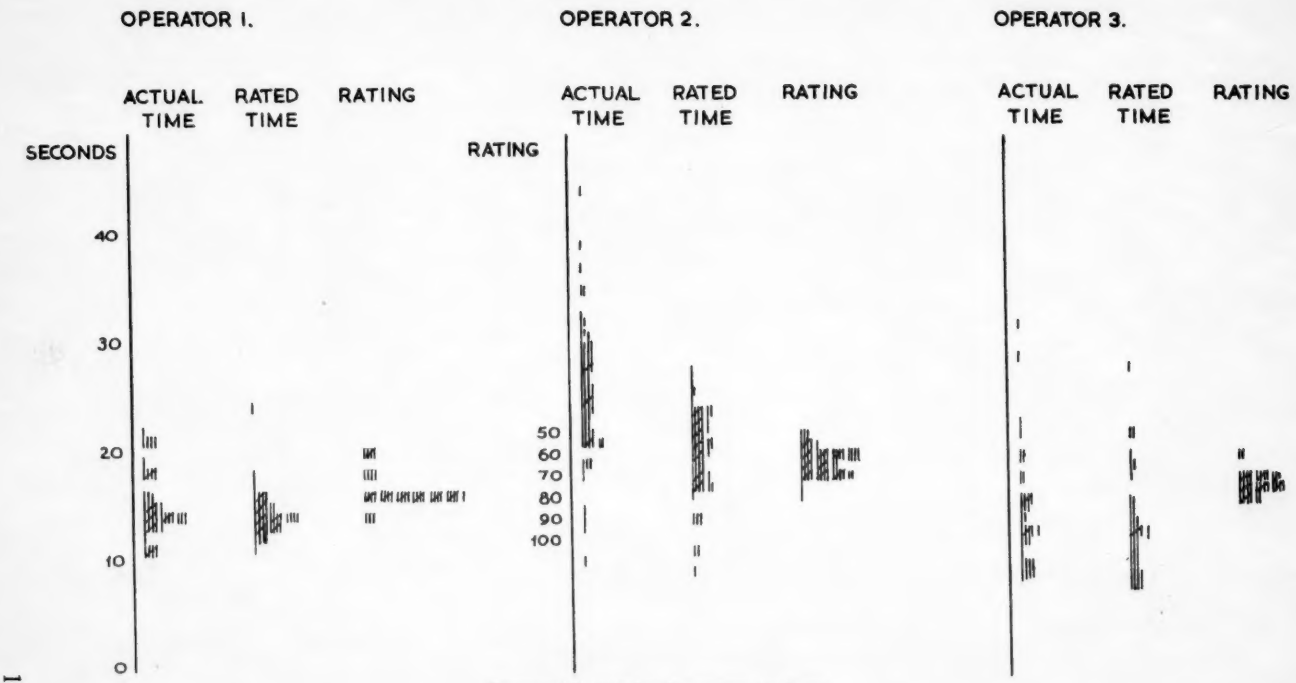

FIGURE 15. *Works C, coning, tie knot.*

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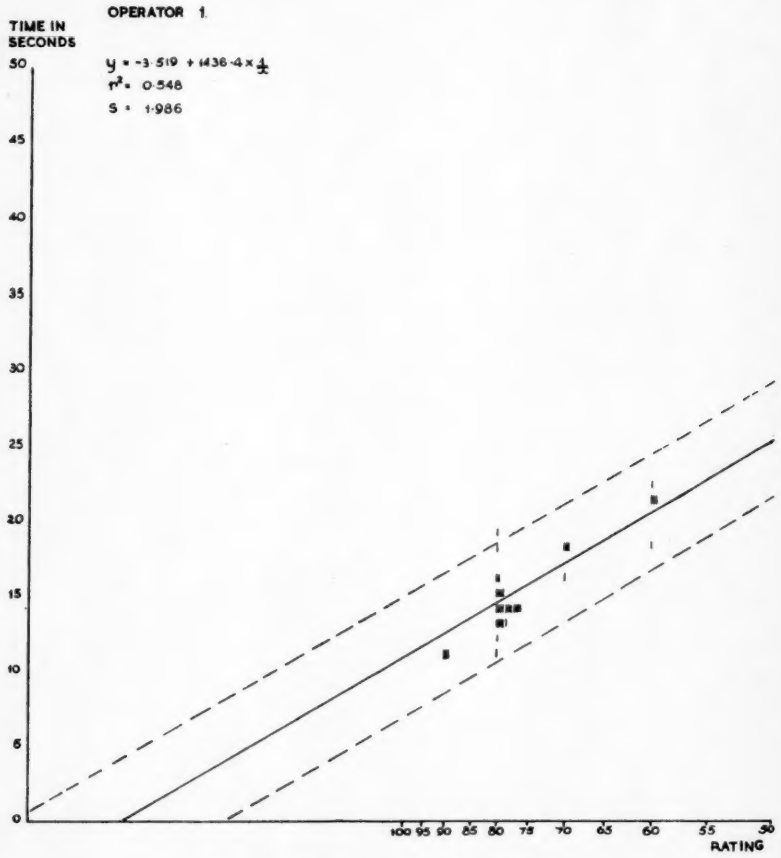

FIGURES 16. *Works C, coning, tie knot.*

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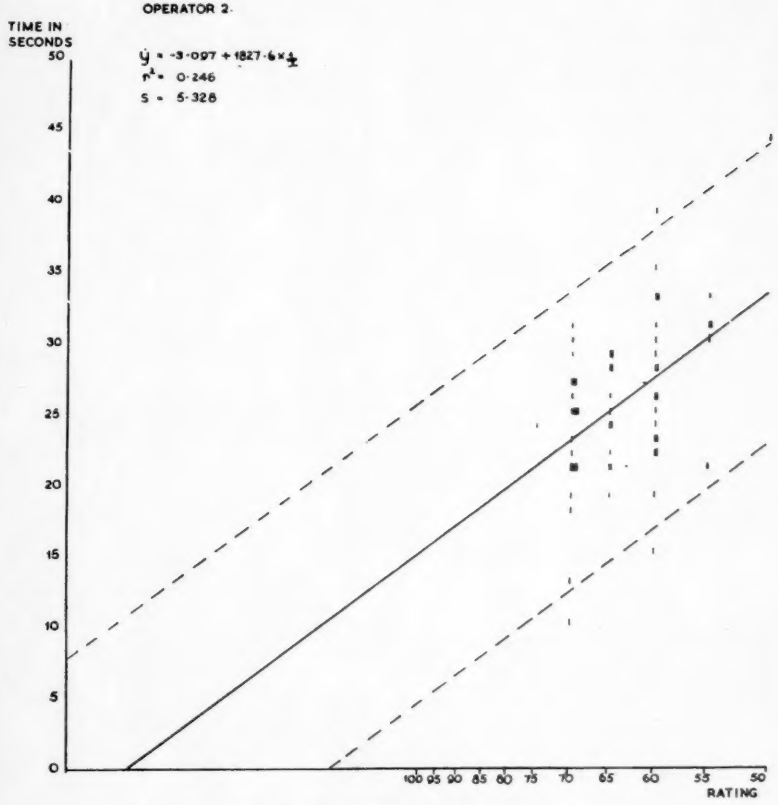

FIGURE 17. *Works C, coning, tie knot.*

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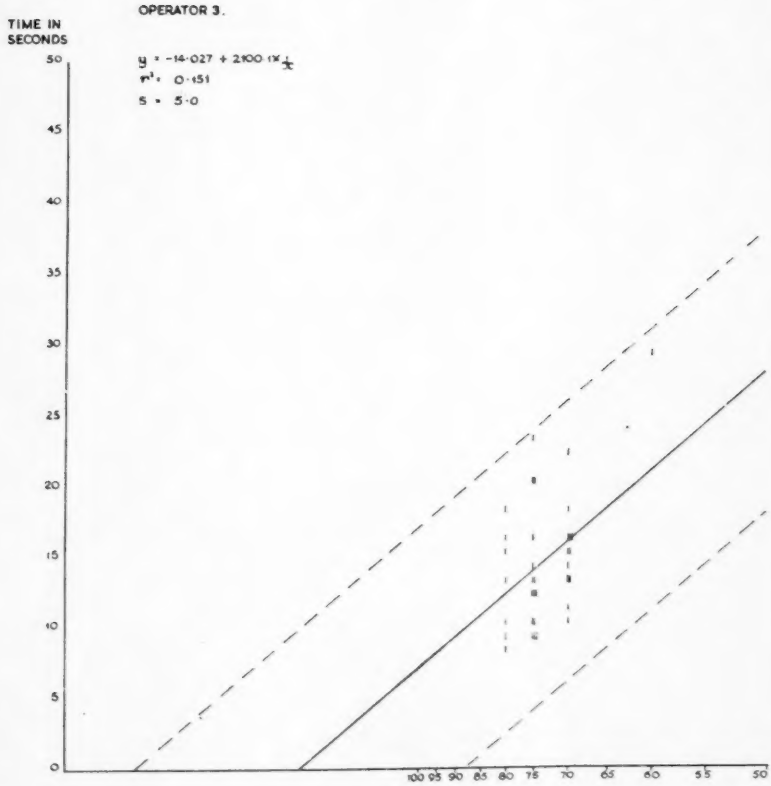

FIGURE 18. *Works C, coning, tie knot.*

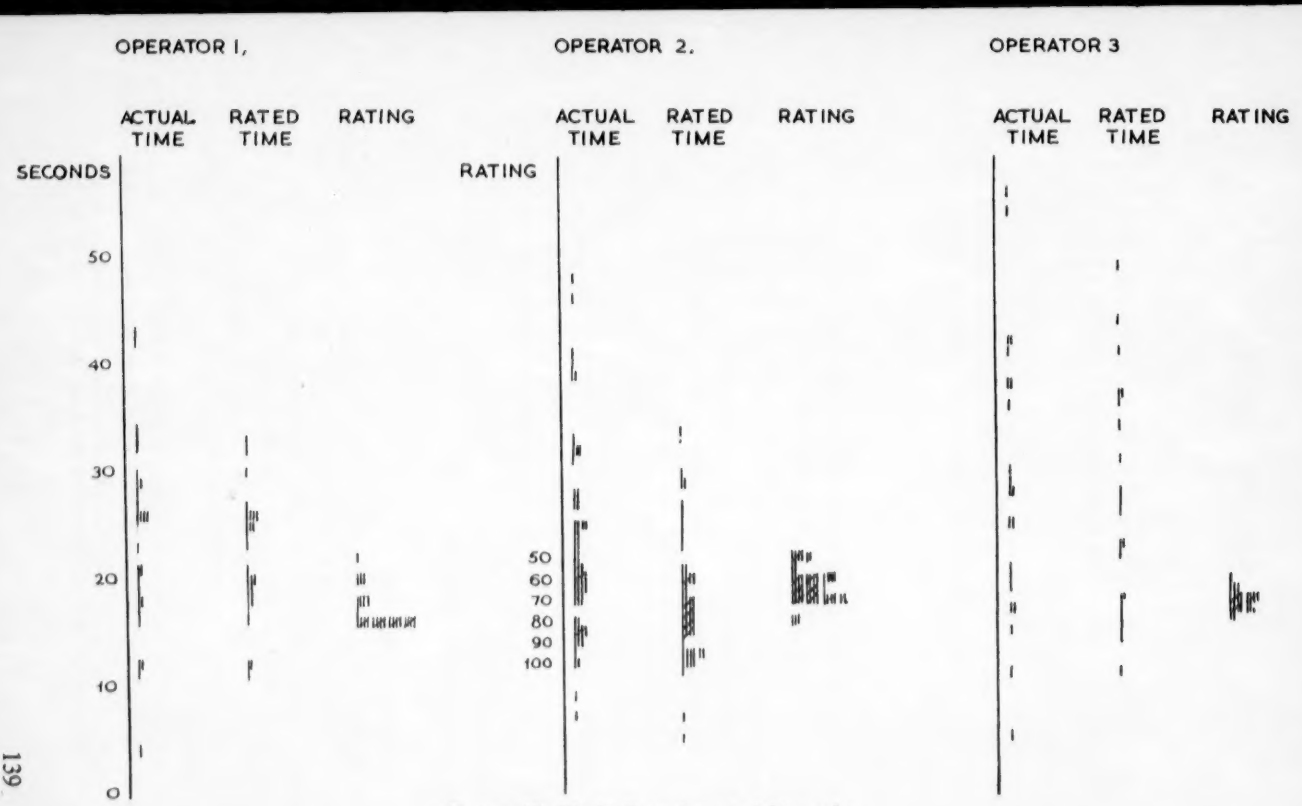

FIGURE 19. *Works C, coning, straighten cake.*

A STOCK MODEL

by

R. R. P. JACKSON*

IN certain industries customers demand speedy delivery of a large variety of products, usually in amounts considerably smaller than the "economic" manufacturing size. To meet this situation stocks are held, and the problem is to obtain an indication of the cost of stock holding, of the class of "small orders" that is profitable, and of the amount of stock to hold. This paper develops one possible model and its approximate solution; it is an abstract model derived for a practical problem.

Model

This problem is characterized by an input into the "stock reservoir" controlled by the industry, and an output largely dependent on the vagaries of the customer. It can easily be seen that having decided upon a "stock level" for each product, one ideal situation would be to commence holding a product at that level, replacing at that point when all the stock had been sold. However, bearing in mind the "randomness" of sales, and the possible perturbations of the manufacturing cycle times, it is unlikely that this situation could ever be reached. In general, erring on the safe side, replacements occur before stock has run out. The effect of this overlap is simply to increase the stockholding temporarily above the required level and so increase the cost of stock holding. Thus, if considerations are limited to the case of "no-overlap" a minimal cost of stock holding will be obtained. This situation will now be considered mathematically.

Cost of Stock Holding

Suppose that the initial stock at a given time $t = 0$, is N units. Further suppose that the distribution of issues is random with a mean of μ units per unit of time. Then assuming that stock replacement of N units occurs exactly at that point when the stock-level has dropped to zero, it is only necessary to consider the sequence of events of one cycle from N down to 0 units, since each cycle is formally equivalent.

Let $P_n(t)$ be the probability that at time t there are n units left in stock. The probability that in a small time δt a unit of stock will

* Central Work Study Department, Imperial Chemical Industries Ltd.

be required is asymptotic to $\mu\delta t$. The equations of the process are then found to be:

$$\frac{\partial P_N}{\partial t}(t) = -\mu P_N(t), \tag{1}$$

$$\frac{\partial P_n}{\partial t}(t) = \mu P_{n+1}(t) - \mu P_n(t) \qquad N > n > 0. \tag{2}$$

Solving equation (2), and by iteration the solution of these equations is found to be

$$P_n(t) = \{[\mu t]^{N-n}/[N-n]!\}\, e^{-\mu t} \qquad N \geqslant n > 0. \tag{3}$$

Using the normalizing equation

$$\sum_{n=0}^{N} P_n(t) = 1, \tag{4}$$

it is found that

$$P_0(t) = 1 - \sum_{n=1}^{N} \{[\mu t]^{N-n}/[N-n]!\}\, e^{-\mu t}. \tag{5}$$

In words, the chance of having any particular numbers of stock units in stock at any time t is given in equations (4) and (6).

Now let the basic value of one stock unit be b, and the cost of holding it for unit time be sb. (This figure comprises the cost of insurance, depreciation, handling, clerical work, and so on; in some quarters it is supposed to be 1 per cent of the cost of the stock per month held, but in others twice or even more times this figure is quoted.)

Then the cost of operating an initial and replacement stock of size N units for the above process is:

$$\begin{aligned} CN &= \sum_{n=1}^{N} \int_{t=0}^{\infty} sbnP_n(t)\, dt, \\ &= sb \sum_{n=1}^{N} n/(N-n)! \int_0^{\infty} (\mu t)^{N-n} e^{-\mu t}\, dt, \\ &= sb \sum_{n=1}^{N} \{n/(N-n)!\}\{\Gamma(N-n+1)\}/\mu, \\ &= sb \sum_{n=1}^{N} n/\mu, \\ &= sb\, N(N+1)/2\mu. \end{aligned} \tag{6}$$

Hence the additional cost per unit of stock is

$$(N+1)sb/2\mu \text{ pence,} \tag{7}$$

and for given s, b and N this can be graphed for any μ.

Cost of Manufacture

It can be seen that in addition to the cost of stock holding calculated above, the cost of the manufacture of N replacement units must be included. This figure can usually be obtained from the works technical or cost sections for different values of N. Suppose, for instance, that the following figures are given:

(1) The average usage per week = 200 units.

(2) The cost per unit when manufactured in lots of 100, 200, 400, 800, 1,600 is 60d., 54d., 50d., 49d., 48·5d., respectively, and

(3) estimated value of $s = 0{\cdot}25$ per cent per week.

Then Table I can be calculated.

TABLE I

Stock units	100	200	400	800	1,600
Basic price d./unit	60	54	50	49	48·5
Stock Holding cost/d./unit	3·8	6·8	12·5	24·5	48·5
Total cost	63·8	60·8	62·5	73·5	97

It is clear that from the cost point of view manufacturing 200 units weekly would give approximately the best result. However, work loading and other considerations may suggest that making 400 units every fortnight would be more convenient, and since the cost is only 1·7d./unit more, this may be acceptable. It should be noted that when considerations of this type are being undertaken, an eye should be kept on the risks of "running out of stock" in the average cycle time, which can be calculated from (3) and (5).

It is suggested that for each particular application a series of curves for given N, of cost of stockholding (6) against varying average output rates be drawn.

ACKNOWLEDGEMENT

I am indebted to Imperial Chemical Industries Limited for permission to publish this paper.

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ABSTRACTS AND REVIEWS

The Social Character of Technology.
TOM BURNS
Impact, 7 (3), *September*, 1956.*

Good presentation of handling technological development: of interest to O.R. group leaders.

A technical Consideration of the Cybernetic Analogue for Planning and Programming.
STAFFORD BEER.
Process Control and Automation,* 3 (8) and (9), *August*, *September*, 1956.

Paper read to the Congrès International de Cybernetique. Title precisely describes long paper, "curious" rather than important, though four diagrams, Production Control, Basis of O.R. model, O.R. techniques, and Science of Control are interesting.

Conference on Linear Programming.
Computer Sales Dept., Ferranti, Ltd., Manchester.

Proceedings of one-day conference on Linear Programming convened by Ferranti. Six papers on the various computational techniques including Simplex and Method of Leading Variables, for finding maximum or minimum of a linear set subject to constraints: one by Dr. Prinz gives concise statement of Simplex with some interesting results on Manchester machine, but others less realistic.

Linear Programming on the Ferranti Mark I Computer.
Computer Sales Dept., Ferranti Ltd., Manchester.

Brief description of linear programming, with five examples from industrial field showing feasibility of programming to digital computer otherwise prohibitively long computations. One programme in detail.

An Introduction to the Principles of Analogue Computing.
A. D. JEFFERY.
Process Control and Automation,* 3 (9), *August*, 1956.

Useful summary of general use of analogue computers, showing principles, technical methods and formulation.

Actuarial Mathematics applied to Study of Component Failures.
MORICEAU, J. (In French.)
L'Etude de Travail,* 66, *September*, 1956.

Contribution to symposium by Société de Metallurgie et Mécanique. Author considers use of actuarial mathematics to study of component failures in complex electronic devices; uses Makenham's

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formula for expected life of component and proceeds to calculate probability of machine failure. Also describes use of methods to set maintenance schedules and stocks of spares.

Quantities in Batch Production.
EILON, S.
Engineering, **182**, 7, *September*, 1956.

Discussion of lot-size in batch production with formula for minimum cost lot for one or more products. Alternatives to fit multi-product schedule constraints are analysed on several considerations. Detailed with Bibliography.

Historical and Scientific Background of Automation.
COALES, J. F.
Engineering, **182**, 21, *September*, 1956.

Traces automation from earliest times to present day with good examples and details.

*The above abstracts are contributed by ORbit (Operational Research) Ltd. Copies of publications marked with an asterisk can be obtained on loan from that firm at Empire House, St. Martin's le Grand, London, E.C.*1.

Operations Research for Management, Vol. II.
Edited by JOSEPH F. MCCLOSKEY and JOHN M. COPPINGER.
Johns Hopkins Press, Baltimore; Oxford University Press, London, 1956. 63*s*.

Volume I of this work, published in 1954, was reviewed in *Operat. Res. Quart.*, **6** (2), 88. That volume presented 23 papers by different authors presented to the Johns Hopkins Operational Research Seminar during 1952–3. Volume II, now reviewed, comprises some case histories, methods, and a section on information handling, presented at informal seminars on operations research at the Johns Hopkins University during 1953–4 and 1954–5.

The first part of Volume II on case histories is rather more readable than the corresponding parts of Volume I, the contents of which were, in many respects, too well known. It is true that some of this present volume is a reprint of articles in *Operations Research* and one an expansion and revision of an article from this journal, but these contributions are still interesting and the other contributions are fresh. Taken together they form a valuable, compact set of case histories. Whether management faced with these case histories will feel that they have similar problems to solve is doubtful, for the canvas in some cases is on a vast, industry-wide scale, or public service scale which may not have a counterpart in industry. Nevertheless, these papers are well written and interesting documents and can be highly commended.

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The second part of the volume, methodology, is perhaps not quite so useful. Some chapters are in such general terms as hardly to justify inclusion under methods. The more specific chapters may perhaps have suffered in this reviewer's reading from his own personal interests but nevertheless it seems likely that other readers may feel the same way about these contributions. One contribution at least, on the travelling salesman problem, has nearly lost any appeal it had for one reader. The chapter on failure of equipment is an interesting academic exercise but it should point the way to some practical applications. The chapters on queueing problems, and Monte Carlo methods and operational gaming in industry, although useful, are not likely to appeal to managers.

The last part of the book, on information handling in organized groups, is also likely to be difficult reading for managers. This is comparatively new work and not a great deal has previously been published. It can be commended to the professional O.R. man as an attempt to break into a difficult field.

Some errors have been noted. On page 168, line 9.

$$\lambda = \cdot 46 \text{ should be } \lambda = \frac{66}{180} = \cdot 37,$$

and on line 20 it appears that the value 49 should be 43, as in line 22.

Again, on page 170, the definition given for $P_n(T)$ seems to be inconsistent with the equations appearing on subsequent pages.

However, the work as a whole is a "must" for any operations analyst's bookshelf. It contains ideas, methods, case histories that the professional must be aware of, even if he need not perhaps always bring himself up to date on the details. Whether the book is a satisfactory book for management is less certain. Any volume of this sort must be to some extent a compromise between a compendium for the manager containing matter familiar to the professional, and a compendium for the professional likely to be unintelligible to the manager. This volume contains something for both.

K. P.

Bollettino del Centro per la Ricerca Operativa

We have received No. 2 of this publication, which contains 45 pages together with advertisements and abstracts. It contains two papers in Italian, one on linear programming applied to a distribution problem, and one describing a theoretical scheme for the classification of learning processes. There are also letters and items of news.

Studies in the Economics of Transportation.

MARTIN BECKMAN, C. B. MCGUIRE and CHRISTOPHER B. WINSTEN. Introduction by Tjalling C. Koopmans.

Published for Cowles Foundation for Research in Economics, Newhaven, 1956 (*Yale University Press*). (*London, Geoffrey Cumberlege: Oxford University Press*). *Pp. xix*+232, *illus.*, 32*s.*

This book is divided into sections, the first on highway transportation, the second on rail transport. Five chapters are devoted to highway transport. The first deals at some length with road and intersection capacity and is mainly a review of existing knowledge on the effect of traffic flow on speed and delay with prominence given to the work of Normann, Walker and others. The authors do give however an interesting theoretical model for analysing delays to vehicles in a minor traffic stream which crosses a major traffic stream having right of way. They assume that time is divided into discrete points which may be either red-points, indicating inability to cross or green-points indicating free passage, not more than one vehicle being able to cross at any one time point. They also assume that vehicles arrive with a probability of α per 100 time points and that in a random selection of numbers from 1 to 100, the selection of numbers between 1 and α denotes an arrival, and the selection of number α to 100 a non-arrival at a given time point. An arrival at a green point then gives free passage to the vehicle and an arrival at a red point gives delay. They apply this model to intersections (with or without traffic lights) to derive the average delay to vehicles in the minor stream and to derive average speed in the case of a stream of faster vehicles passing slower vehicles on a road. It is doubtful however whether the model put forward will be as useful in this field as the more empirical work of Webster carried out in this country (summarized in *Road Research* 1953 and 1954) whose formula produces results corresponding closely to observed delays.

In the following chapter a more fully economic approach is developed. The chapter is concerned with the demand for road transport facilities by road users and puts forward a supply (or cost) and demand analysis for the determination of traffic flow and road conditions in a given situation, the supply (or cost) sides of the equations being supplied by the flow/speed and flow/delay relationships discussed in Chapter 1.

The equilibrium of demand for and supply of road transport facilities both on a single road and on a network of roads is then considered, equilibrium being reached when on each road the demand for transport, and the flow thus established, give exactly

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those road conditions which would call forth that demand. Some attention is given to the stability of the equilibrium thus established, i.e., the tendency of the demand and flow/speed (or flow/delay) functions to restore equilibrium after small deviations from it have occurred. The authors conclude that in most cases equilibrium is likely to be stable, a conclusion amply confirmed by the fact that instability of flows, speeds and delays on roads is a rare phenomenon.

In what is probably the most important contribution in the book, the fourth chapter discusses efficiency and considers how far the individual road user, left to his own free decision, will make the most efficient use of a given road network. The authors show that, because he imposes costs on other road users which he ignores when making his decision, the road user by free choice will not make the most efficient use of the road network. Faced with a choice between a short congested route and a longer less congested route for example, he will choose the former if his costs are least on that route, even though the total costs attributable to his journey are greater on that route. Thus there will tend to be too much traffic on short congested routes and too little on long uncongested routes and to remedy this and obtain a social optimum the authors propose an efficiency toll, increasing with the degree of congestion (and thus with the divergence between private and social costs). With such important implications for road policy in congested areas, it is disappointing to see the authors dismiss the efficiency toll as impracticable without fully considering alternative ways of using the price mechanism to achieve efficiency, e.g., special licences. The section on road transport ends with a discussion of various unsolved problems in the field of capacity, equilibrium and finance.

Seven chapters are devoted to railroad transportation. They are principally concerned with the utilization and the grouping (inter-trains) of rolling stock and witn the routeing and scheduling of trains, using examples and models in order to illustrate the principles that must be followed for economic operation.

If any criticism can be made of this book it is that its work on highway economics is too ambitious. The authors make few simplifying assumptions and approximations in their analysis and consequently with the number of unknown factors and relationships in this field, they find it difficult to produce conclusions. The book performs a valuable service however in bringing out and facing the problems and complexities that are inevitable in this subject, and in showing that the problems of highway economics cannot be satisfactorily solved by the accounting techniques which still dominate this field.

D. J. R.

A MARKOV CHAIN PROCESS IN INDUSTRIAL REPLACEMENT

by

M. W. SASIENI*

IN the manufacture of rubber tyres a machine is used which contains two "bladders", and which produces one tyre on each bladder simultaneously. Bladders can fail in service, a faulty bladder being discovered when a tyre fails to pass inspection. Thus there is a cost of a scrap or "second" tyre associated with a bladder which fails in service. Further, when a bladder fails the machine must be stripped down before replacement can be made, a process requiring fitters, labour, and production time with an associated cost. However, once a machine has been stripped, both bladders can be replaced at the same time. The cost of replacing the second bladder is virtually the cost of purchasing the bladder alone.

It is found that the probability of failure is a function of the number of tyres made with a particular bladder. It thus seems likely that the cost of replacing bladders could be kept at a minimum with the following policies:

(A) Replace bladders which have made a predetermined number of tyres without failure.

(B) When a machine is stripped to replace one bladder, replace the other bladder if it has produced more than a given number of tyres.

For brevity the number of tyres produced on a given bladder will be called the "age" of the bladder. Normal production records show the number of tyres produced on each bladder, and a curve of the probability of failure with age is readily obtained.

It is the purpose of this paper to show how the ages required in A and B can be determined so as to minimize the average costs of replacing bladders per useful tyre produced; that is, to minimize the sum of:

(i) The costs of stripping a machine for bladder replacement.

(ii) The costs of lost production time due to a bladder failing and being replaced on a production shift.

(iii) The costs of "second" or scrap tyres produced.

(iv) The costs of purchasing bladders.

It will be shown that the system can be characterized by a Markov chain process, and that for any given policies of types A

* Case Institute of Technology, Cleveland, Ohio, U.S.A.

and B, the system ultimately becomes stable, and the average cost per tyre becomes constant. An example of the computation of costs associated with each policy is given and the best policy is determined

Notation

We shall use the following notation:

p_i = probability that a bladder, aged i, will fail on the next tyre. The state of a pair of bladders in a machine may be characterized by their ages, (i,j); ${}_{ij}p_{rs}$ is the probability that on producing a pair of tyres the bladders will pass from state (i,j) to (r,s).

h_{ij} is the probability of ultimately finding the system in the state (i,j).

m is the predetermined age in policy A, i.e. all bladders are to be replaced on reaching age m, whether or not they have failed.

$n(\leqslant m)$ is the predetermined age in policy B, i.e. the second bladder will be replaced if its age is n or more, whenever the first bladder is replaced.

c_1 is the cost of purchasing a bladder.

c_2 is the cost of the scrap produced if a bladder fails on a production shift.

c_3 is the labour cost of stripping a machine to replace one or both bladders.

c_4 is the cost of lost production time if a bladder has to be replaced during a production shift.

Mathematical Section

We first compute a matrix $P = [{}_{ij}p_{rs}]$ (ij along the top; rs down, in the same sequence). The probabilities ${}_{ij}p_{rs}$ can be computed from the table below. From the state (i,j) the system can move to one of four possible states, viz.:

$$(i+1,j+1),\ (i+1,0),\ (j+1,0),\ \text{or}\ (0,0),$$

where zero denotes replacement of a bladder, either because of failure or because of policy.

It is not possible for the system to pass to any other state and the remaining ${}_{ij}p_{rs}$ are all zero.

It can be shown that with such a matrix, the system ultimately reaches a steady state. (See, for example, Feller, *An Introduction to Probability Theory and its Applications*, chapter 17 on.) We can think of the steady state most easily in terms of a large number of machines. If the values m,n are used consistently, then ultimately the proportion of machines in each of the possible states (i,j) will

become independent of time. These proportions, which we denote by h_{ij} are the probabilities of finding a single machine in the state (i,j). Given the h_{ij} we can use the matrix P to compute the expected number of single and double replacements and the number which arise because of failure. We can then determine the cost associated with any pair of ages (m,n).

Consider the system before and after a pair of tyres are made. Since ultimately the proportions in state (i,j) remains constant we must have $H = PH$ where H denotes the column vector of $[h_{ij}]$*, i.e.

$$(P-I)H = 0 \tag{1}$$

where I is the unit matrix.

This represents a set of homogeneous linear equations in the h_{ij} and for a non-trivial solution it is necessary and sufficient that the determinant $[P-I]$ vanish. This is clearly the case since the elements of each column in P are probabilities adding to one.

In addition, we have the equation

$$\sum_{ij} h_{ij} = 1. \tag{2}$$

Thus the h_{ij} are completely determined.

Because transitions can only take place between certain states, equations (1) reduce to

$$\begin{aligned} h_{00} &= \textstyle\sum_{rs} p_{00} \cdot h_{rs}, \\ h_{0j} &= \textstyle\sum_{i} {}_{i,j-1} p_{0j} \cdot h_{i,j-1}, \\ h_{ij} &= {}_{i-1,j-1}\, p_{ij} \cdot h_{i-1,j-1} \qquad i \neq 0, j \neq 0. \end{aligned} \tag{3}$$

Equations (3) can easily reduce to $(m+1)$ equations in h_{00}, h_{01}, h_{02}, ..., h_{0m} only.

The average cost of purchasing bladders, per cycle, will be

$$c_1 \left\{ 2h_{00} + \sum_{j=1}^{m} h_{0j} \right\}. \tag{4}$$

The average cost of scrap produced will be

$$c_2 \sum_{i,j} h_{ij}(p_i + p_j). \tag{5}$$

The average cost of lost production time will be

$$c_4 \sum_{ij} h_{ij}(p_i + p_j - p_i p_j). \tag{6}$$

* The order of the rows in H is immaterial, so long as they are the same as the columns in P. We later discuss the most convenient order for computation.

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The average cost of stripping machines to replace bladders will be

$$c_3 \sum_{i=0}^{m} h_{0i}. \tag{7}$$

We wish to choose (m, n) so as to minimize the cost per tyre produced.

The number of good tyres per cycle is 2, less the scrap produced, i.e.

$$2 - \sum_{ij} h_{ij}(p_i + p_j). \tag{8}$$

Hence we wish to minimize

$$\frac{(4)+(5)+(6)+(7)}{(8)}. \tag{9}$$

It will be realized that each pair of ages (m, n) will require a separate calculation of h's and associated costs. Arithmetical labour can be minimized by dividing the total possible life-span of bladders into relatively few intervals and calculating costs for a series of policies. The minimum costs can then be found by curve fitting and interpolation.

TABLE I

PROBABILITIES OF MOVING FROM STATE (i, j) TO (r, s)

State (r, s)	Probability, ${}_{ij}p_{rs}$	Condition on (i, j)
$(i+1, j+1)$	$(1-p_i)(1-p_j)$	$i \leqslant jm$
$(i+1, 0)$	$p_j(1-p_i)$	$n > i; j < m$
	$1-p_i$	$n > i; j = m$
$(0, j+1)$	$p_i(1-p_j)$	$i \leqslant j < n$
$(0, 0)$	$p_i p_j$	$i < n, j < m$
	p_i	$i < n, j = m$
	p_j	$i > n, j < m$
	1	$i > n, j = m$

Numerical Computation

We conclude with an example of numerical computation based on notional figures.

Suppose the probabilities of failure of a single bladder are as in Table II.

We next compute the matrix P for the case where $m = n = 6$, i.e. replacements are only made on failure. It is convenient to show the first state (ij) along the rows and the state to which it moves

(r, s) down the columns. Now all the states, not of the form $(0, i)$, derive from one of the states (0, 0), (0, 1), (0, 2), (0, 3), (0, 4), (0, 5), or (0, 6) by successive operations in which no bladder fails. For example,

TABLE II

PROBABILITIES OF FAILURE

Age x	0	1	2	3	4	5	6	
p_x	0	·1	·2	·4	·6	·8	1·0	failure
$1-p_x$	1	·9	·8	·6	·4	·2	0	survive

TABLE III

VALUES OF h_{001}, h_{01} – – h_{06} FOR VARIOUS POLICIES

Policy	h_{00}	h_{01}	h_{02}	h_{03}	h_{04}	h_{05}	h_{06}
$m = 6$ $n = 6$	·0549	·1131	·0988	·0641	·0624	·0189	·0038
$n = 5$	·0589	·1090	·0991	·0646	·0621	·0189	—
$n = 4$	·0793	·0926	·0968	·0674	·0618	—	—
$n = 3$	·1472	·0505	·0818	·0727	—	—	—
$m = 5$ $n = 5$	·0559	·1148	·1016	·0638	·0639	·0192	—
$n = 4$	·0764	·0942	·1028	·0668	·0626	—	—
$n = 3$	·1449	·0520	·0829	·0756	—	—	—
$m = 4$ $n = 4$	·0608	·1335	·0987	·0699	·0725	—	—
$n = 3$	·1314	·0595	·1075	·0801	—	—	—
$n = 2$	·2337	·0138	·0622		—	—	—

(3, 4) derives from (0, 1) via (1, 2) and (2, 3). It is convenient to group the states according to which of the states $(0, i)$ it derives from. The matrix P is shown on page 154. In this table p denotes the probability of death at the age shown above; q is the probability of survival. The row headed "probability" shows the probability of passing in the shortest number of steps from the base state to the state shown. It is also the ratio of h_{ij} to the base state h_{0i} or h_{0j}. The elements of the matrix can be computed from the p, and the "probability" row is inserted later.

Let H_x denote the column vector $[h_{00}, h_{01}, \ldots, h_{0x}]$ and P_{mn} denote the matrix of coefficients when equations (3) are expressed in terms of the base states only.

Thus equations (3) become $P_{mn} H_n = H_n$.

The elements of P_{66} can now be derived by reading along the row $(0, i)$ multiplying the elements by the corresponding element in the probability row, and summing for each base state. The equation $\sum_{ij} h_{ij} = 1$ is transformed into an equation of the form $\sum_{i} \alpha_i h_{0i} = 1$ by summing the probability row over each base state.

P_{65}, P_{64}, etc. can now be derived from P_{66} by successively adding the last row of the preceding matrix to the first row, and

successively omitting the last row and column. The equation corresponding to $\sum_i \alpha_i h_{0i} = 1$ is obtained by omitting the states which cannot occur if $n < 6$.

It is probably best to go back to the matrix P in order to derive P_{55}, P_{44}, etc.

The resulting sets of equations were solved by inverting the matrices.

We now assume the following costs are given:

the cost of purchasing a bladder, $c_1 = 50$,
the cost of scrap, per scrapped tyre, $c_2 = 1$,
the labour cost of stripping machines, $c_3 = 2$,
the cost of lost production time, $c_4 = 3$,

and deduce the amounts per cycle and average cost per useful tyre produced, using (4), (5), (6), (7), (8) and (9). The results are given in Table IV.

TABLE IV

AMOUNTS PER CYCLE AND COST PER TYRE

Policy	Amount per cycle: Stripping	Purchasing	Scraps	Lost production time	Average cost per tyre
$m = 6$ $n = 6$	·416	·471	·471	·416	17·22
$n = 5$	·413	·472	·468	·413	17·05
$n = 4$	·397	·477	·454	·398	17·01
$n = 3$	·352	·499	·410	·352	17·06
$m = 5$ $n = 5$	·419	·475	·458	·406	17·03
$n = 4$	·402	·479	·447	·394	16·99
$n = 3$	·355	·500	·407	·351	17·06
$m = 4$ $n = 4$	·435	·496	·407	·366	17·07
$n = 3$	·478	·510	·384	·338	17·11
$n = 2$	·309	·544	·345	·293	17·55

The optimum policy, which gives the lowest cost per tyre, is thus $m = 5$, $n = 4$.

However, if the ages represent, say, hundreds of tyres, we can interpolate using a second difference formula, and differentiating it for a minimum.

This gives:

for $m = 6$, the minimum cost of 17·098 when $n = 4{\cdot}06$,
for $m = 5$ we obtain a minimum cost of 16·989 when $n = 4{\cdot}14$.

The minimum cost for $m = 4$ is clearly 17·07 when $n = 4$. Interpolating again gives an absolute minimum cost of 16·988 for $m = 4{\cdot}9$. A straight line interpolation between $n = 4{\cdot}14$ when $m = 5$ and $n = 4{\cdot}00$ when $m = 4{\cdot}00$ gives the optimum $n = 4{\cdot}12$.

TABLE OF TRANSITION PROBABILITIES—THE MATRIX p

| "Base state" | (0, 0) | | |

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A little caution is required in interpreting these values for m and n. Our original policies were to replace after the relevant ages exceeded m and n. That is, we replaced when the ages were $m+1$ and $n+1$. Consequently, if we are now working with units of one hundred, our optimum policy is to replace immediately following the cycle when the older bladder reaches 590, and to replace the second bladder if its age is not less than 512.

Of course, in this example the interpolation is hardly worth while, but it is included for purposes of illustration.

SCALE FACTORS IN COAL-MINING LABOUR INDICES

by

T. T. PATERSON*

As mining engineering techniques advance collieries tend to become larger, as is shown in Table I:

TABLE I[1]

OUTPUT PER MAN-YEAR BY SIZE OF MINE

Size	Tons 1947	1948	1949	1950	1951	1952
250– 499	242·7	242·8	258·3	265·8	272·6	271·5
500– 749	255·9	258·4	275·4	282·7	294·3	286·2
750– 999	252·3	266·8	273·4	289·8	294·2	295·6
1,000–1,499	280·1	283·2	298·5	313·7	324·7	316·1
1,500–1,999	263·6	285·6	307·0	321·8	325·6	323·8
2,000–2,499	265·3	277·1	286·3	301·8	306·7	309·4
2,500–2,999	261·5	291·3	295·6	302·6	310·4	294·9
3,000 and above	288·4	243·6	270·9	280·7	272·8	282·4
All	262·2	272·3	286·4	298·4	306·7	303·0

Productivity increases with size up to an optimum in mines employing about 2,000 men, thereafter falling in the few largest mines—confined to Yorkshire and the Midlands. (The reason for the fall in the few largest mines, absent in Wales and Scotland, will be adduced later.)

If mines can be divided into small and large, those employing less than 500 men and those employing more than 1,000 (following Revans[2]), then it can be said that the productivity in large mines is greater, roughly by over 30 tons per man-year, than in small mines. This is the all-powerful factor in the National Coal Board's drive for greater production. Nevertheless, there are certain drawbacks to the increasing size of mines.

* Department of Social and Economic Research, University of Glasgow.

(1) *Attendance.* Comparing the mean absenteeism in all mines employing less than 100 men with that in all mines employing over 1,000 men, it can be seen (Table II) that there is a tendency for absenteeism to be greater in the larger mines. Trist and Bamforth[3] have shown that the new methods of coal-getting (mainly longwall) lead to less coherent, as well as larger, work-groups at the coal face than used to be the case in the older systems. In these older systems the inter-dependence of the men, in the fullest significance of the word "inter-dependence"—technical and social—demands better attendance; a miner knows that if he is absent his "buttie" is likely to suffer. In the modern system a face-worker can be absent and know that his mates are not likely to suffer in any way, and will even condone his absence.

TABLE II

ABSENTEEISM IN SMALL AND LARGE MINES, 1942

Coalfield	Number of Mines Smaller	Number of Mines Larger	Average Absenteeism Smaller (per cent)	Average Absenteeism Larger (per cent)
Scotland	108	12	5·65	7·50
Northern	92	46	6·72	8·41
Yorkshire	42	56	10·02	13·73
North Midlands	27	35	9·05	14·19
Midland	61	20	7·71	13·45
Lancashire and North Wales	41	21	7·43	13·12
South Wales	79	29	7·51	10·98
Great Britain	450	218	7·15	11·66

(Source: Ministry of Fuel and Power)

TABLE III[5]

TONS LOST PER MAN IN DISPUTES, 1949–53

Men on Books	South Wales No. of Pits	South Wales Tons lost per Man	Yorks. No. of Pits	Yorks. Tons lost per Man
0– 500	35	9·7	28	5·9
500–1,000	40	12·1	25	9·0
1,000–1,500	23	15·0	20	16·4
1,500–2,000	6	23·4	21	12·8
2,000–2,500	—	—	8	16·0
2,500 and above	—	—	12	22·7

This means that, in smaller mines wherein the older systems of working survive, there will be a lesser tendency for men to absent themselves from work. Whether this factor of technical change accounts for the total difference is another matter, but it is likely to be primary in the sense that social groupings alter in coherence as a result of change. (In large collieries, in certain specialised occupations such as "brushing" where small-group working still

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functions, the absenteeism tends to be lower than in the adjacent big groups.)

Many industries are expanding as a result of technical innovations and are increasing the size of their work groups, so this phenomenon is likely to be met in factories.[4] A recent survey of 150 firms by the Research Institute of America showed a significant relationship between size and absenteeism, and also unionization.

(2) *Stoppage of work*. Tonnage of coal lost per man in stoppages of work and go-slows is greater in large collieries.

In small mines the loss is about one ton per man per year, and in those employing more than 1,000 men the loss is between two and three tons. At first this might suggest that "there is a significantly greater tendency for men to join or persist in disputes in large mines than in small",[6] but tonnage loss is not an accurate criterion of what might be called differential strike-proneness.

Some years ago an analysis was made of the coal-mining stoppages in Scotland between 1947 and 1951, according to size of mines.

TABLE IV

NUMBER OF STOPPAGES IN SCOTTISH COAL-MINES BY SIZE, 1947–51

Size	Stoppages per Mine	Stoppages per 1,000 Men
– 100	1·00	15·80
101– 250	2·04	12·87
251– 500	3·36	9·04
501–1,000	4·05	5·46
1,001–2,000	5·35	3·78

There are likely to be five times as many stoppages in a mine employing over 1,000 men than in a mine employing less than 100 men. But ten of the small mines would have four to five times as many stoppages as one mine employing the same number of men. That is to say, man for man, the miners in small mines are more likely to strike, which those of us who live in the West of Scotland know very well.

This is to be expected, for striking is a group phenomenon. In one large mine there are more groups than in one small one, but in a cohort of small mines, of the same total number of men as in one large mine, there are more groups with strike potentiality. In a large mine, because of the technical changes already mentioned, groups are larger than in small mines, and since a large-number strike persists longer than a small-number, the strikes in large mines will tend to be longer. Since this is to be expected from simple theory, these findings for Scotland most probably hold for Wales and Yorkshire as well.

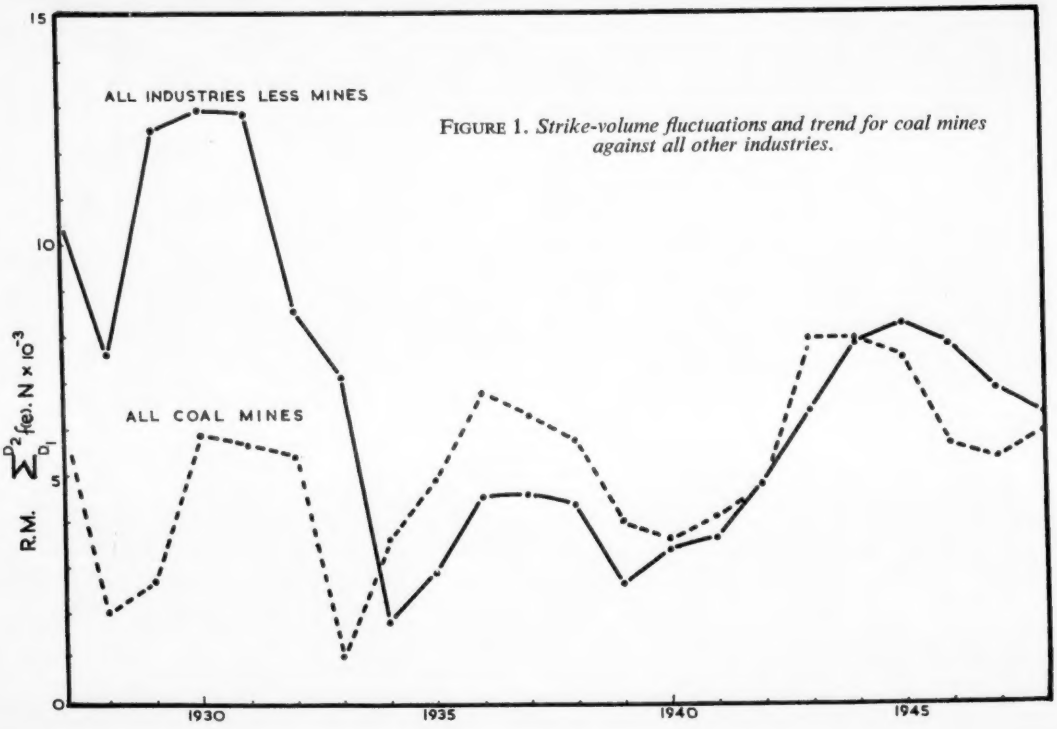

FIGURE 1. *Strike-volume fluctuations and trend for coal mines against all other industries.*

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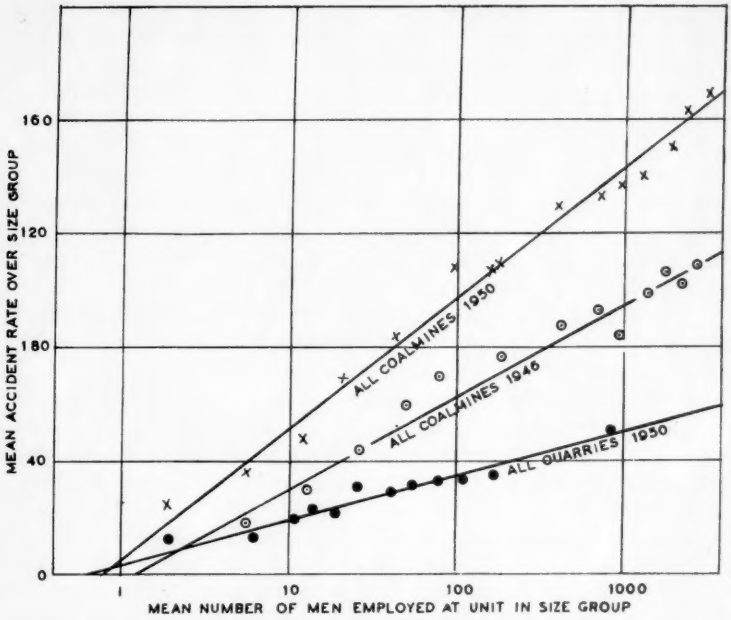

FIGURE 2. *Relation between mean accident rate and log mean size of all coal mines,* 1946–1950, *and of all quarries,* 1950.

What can be said is that miners in small mines have a significantly greater tendency to *join* in strikes than those in large mines, but men in larger mines have a significantly greater tendency to *persist* on strike than those in small mines. Since men in larger mines have a much higher productivity as well, the tonnage lost per man in stoppages in larger mines will tend to be greater. This phenomenon is mainly a function of technical change and of a characteristic of strikes common to all industries and not just to coal-mines.

There are reasons for believing that the inverse relationship of stoppages per mine and per man indicates that the feeling which engenders strikes may not be wholly the outcome of the character of the organization, or state of technical advance of particular mines. The "feeling" or "strike-drive" (or some such word) which brings men out on strike can be comparatively measured by numbers of men per unit duration of stoppage.* This seems to be fairly

* $V=\sum_{D_1}^{D_2} f(e)\,N.$

constant for small and large mines. In other words, the tendency to strike is affected by a factor external to the mine—though, of course, a situation in the mine may itself be the trigger to the strike.[7]

Strike-volume fluctuations and trend calculated for coal-mines as against all other industries over the period 1927 to 1949, give the result in Figure 1. This suggests that fluctuations and trend of strike-volume do not depend on occupation, forms of negotiation, differential organization, wage-rates, and obvious economic factors; which is what Knowles found.[8] He called the relevant factors "imponderables", and these factors appear to affect all miners equally, whether in small or large mines.

(3) *Accidents.* The larger the mine the greater the accident rate as measured in compensatable accidents per standard number of shifts worked. The relationship is logarithmic (Figure 2).

The figure[9] suggests that accidents would be nil in the case of a one-man coal-mine, that is, an act of God would be most unlikely to occur. Such acts appear to be very uncommon in factories, for the vast majority of accidents there are the result of "carelessness" or some such human factor; and the same holds for the mines. The result in Figure 2 is exactly what is to be expected according to theory.[10]

The greater the number of men involved in any social undertaking, or better, the greater the social exposure, the greater the chance of accidents; much as urban areas tend to have higher accident rates than rural. The mere fact that there are more men in a large mine is the cause of the difference; the factor is essentially statistical and has little or no relation to organization except in that if, in larger mines, groups were segregated with little or no social interaction, they would tend to have lower accident rates. In mines where the men are concentrated in one seam they tend to have higher accident rates than those working in similar-sized mines but in more than one seam.[11]

The most significant item in Figure 2 is the comparison between the figures for mines in 1946 and 1950. The accident rate for all mines rose and the increase affected small and large mines nearly equally. That means to say that the absolute accident rate (as distinct from the differential) was raised by some factor which was external to the mines (as already said). The change was not due to nationalization of the mines for this trend has been affecting all mines since the early thirties.[12] The same overall effect can be seen in a single mine where rising trend in accident rate appears contemporaneously and equally in individual sections.[13]

The trend and fluctuations in accident rate seem to be related to the trend and fluctuations in strike-volume, preceding the changes

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in the latter by one year or two. (The available figures for absenteeism in coal-mines show a similar trend, but are suspect. Nevertheless, statistics for factories show a high correlation with accidents and strikes over a short period.) It appears, therefore, that the relative change in absolute accident rate is, like that of strike-volume, a function of a cultural change.

Accident rate at the coal-face can be reduced by social-cultural techniques based upon knowledge of the changes wrought by mechanization (already mentioned) and without reference to organizational structure.[13] This does not mean to say that the comparatively high accident rate cannot be improved by changes in management organization; it means that the causes of the high accident rate are not inherent in the management organization. In so far as technical changes demand greater social exposure in larger mines so will the accident rates be higher; the rates could be reduced by improving the informal structure. Alteration of the existing formal structure is not absolutely necessary (see below).

It can be shown that, where technical and personnel changes are reduced as far as possible, productivity and accidents in mines and factories are inversely correlated.[14] Since the accident rate has been rising over the past generation there should be a related lowering of productivity (and with it, too, rising losses due to increasing stoppages and accidents). This would account for the apparent failure of the Coal Board to reach calculated increases in productivity through technical development—and this quite apart from the "silent strike" of lowered recruitment. The fact that the larger collieries have, in general, increased this productivity more than the smaller despite these drawbacks (cf. Table I), shows that it would be possible to increase the badly needed production if the Board combat the effects of this external cultural factor.

The inter-relationship between absenteeism, accidents, stoppages and productivity can be understood if they are regarded as variants of avoidance or rejection of work, this avoidance being a symptom of frustration in expression of individual needs in a work-group.*

Loss of morale, and therefore team-spirit,† is a shorthand expression, but it must be remembered that loss of team-spirit will give similar results, without necessarily involving loss of morale.

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* Cf. the work of the Tavistock Institute and N.I.I.P. in this country, and the many researches in group dynamics in U.S.A.

† The loose use of the words "morale" and "team-spirit" (capable of proper definition) is most liable to be profoundly misleading, as for instance in the Acton Trust pamphlet *Size and Morale*, 1954. Large corporations such as I.C.I., Unilevers, and Shell, which show some of the phenomena described, have certainly high morale, not low.

It is possible to say that the phenomena described for coal-mines are the result of poor team-spirit even though the morale of miners remains high.[15]

Suggestions

These labour indices discussed so far suggest that though larger mines suffer more from absenteeism and accidents, and lose more coal through stoppages, the causes are mainly technical, statistical, and cultural, and not primarily inherent in the organization of the mines themselves; "primarily" because organization is consequent on technical change.

Revans has shown (op. cit. p. 99) that more men are accommodated in the large mines, not so much by operating more coal-faces, but by raising the average number of men on the faces, i.e. increasing the size of the work group. This is reflected in the span of control of the deputies (the first line of supervision), for the average number of men under a deputy increases with size of colliery. Since larger groups show the negative phenomena described above, it is to be expected that increased span of control can be correlated with them, but is not causal. (The experiment, already mentioned, reduced accident rate in a section without reference to change in supervision.) Technical change is the causal factor in correlatible changes in rates of absenteeism, stoppages, accidents, and productivity, and in differential spans of control; the absolute level of these rates is dependent also upon cultural and statistical factors, and again has no causal relationship to organization.

Larger mines are more effective than the smaller in terms of productivity, therefore the Coal Board must develop in the direction of larger undertakings as it is rightly doing,* just as most industries are doing; and, like these other industries, it must seek to reduce the concomitant drawbacks of absenteeism, strikes and accident rates, which attend technical advance and centre around the size of the work group.

The losses due to strikes can be reduced in two ways. Firstly, the external cultural stress cannot be altered but this stress (showing itself as frustration) more readily reaches the threshold of strike action owing to additional internal frustration.† If the internal frustrations are reduced, then the men are less likely to express cultural frustration in strikes. Secondly, since small strikes are less

* Despite the contrary advice by the Acton Society Trust in *Size and Morale*.

† Studies in Scottish mines and factories have shown that this action-threshold can be computed on an empirical basis.

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prolonged and less costly in tonnage lost, it would be better to allow strike potentiality expression in such forms; and this can be achieved by increasing the coherence and/or by reducing the size of the work groups.

Since high absenteeism, high accident rate and low productivity are intimately related, and conform to the theory of the social threshold, it would seem possible to reduce their effects by action of a kind similar to that for strikes. They are differential group phenomena capable of improvement by action on the work groups, by reducing the groups in size and by increasing interdependence of the members; and since they are fundamentally expressions of avoidance of work, reduction of internal frustration in work would reduce the tendency to avoidance. The classic example of this in practice is the Bolsover Experiment.

Theoretically, therefore, the reduction of group size requires smaller span of control at first level of supervision, that is, increase in number of deputies. Hitherto, the increase of supervision necessitated by larger groups has been achieved by inserting an intermediate line, an overman, between the deputy and the undermanager. But immediately this happens the line of authority is lengthened, that is, communications worsen and this leads to frustration—a finding common in most text-books and work on industrial organization. Revans has discovered that the insertion of this line had some relationship to increased frustration. This is most probably the causal factor in reduced productivity in the largest mines.

It follows, therefore, that if the production line be kept as short as possible, the staff functions now carried out by production-line supervisors must be lifted from their shoulders. The effect of this will be doubly advantageous since the advent of a separate staff line would not only increase the authoritative efficiency of the production line but would also increase the efficiency of the services (the frustration due to bad servicing in mines is a chronic complaint). So, still considering this theoretically, the imperative technical advance in the mines (accompanied by increasing size of work groups and activating marginal frustrations that trigger off the aggression of cultural frustration) requires *simplification of the authority structure in the production line and amplification of the staff line.* This is normally found in industries expanding under the impact of technical advance,[16] and is usually called Production Control and Engineering. What the Coal Board seems to need is the advice of a good industrial consultant with some knowledge of industrial sociology and fundamental theory of industrial administration.

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ASSOCIATION OF MANAGEMENT CONSULTANTS

IT is announced that four of the largest British firms of management consultants have formed the Management Consultants Association. The firms are Associated Industrial Consultants Ltd., Personnel Administration Ltd., Production-Engineering Ltd., and Urwick, Orr & Partners Ltd.

The new Association is intended mainly to develop co-operative research on newer problems, and exchange experience gained in older problems. It will co-operate with other research and professional bodies concerned to improve managerial techniques, and it will make available the results of its research.

COURSE ON QUEUEING AND FLOW PROBLEMS

A COURSE of five weekly lectures on this subject begins at the Mathematics Department of the Northampton Polytechnic, London, on 22 January, at 6.30 p.m. E. D. van Rest, M.A., B.Sc., F.Inst.P. of the Ministry of Supply is the lecturer, and intends to discuss eight types of congestion problems and their industrial and commercial counterparts.

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